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DARWIN AND BRYAN—A STUDY IN METHOD[1]

A FEW days ago a friend, with whom I was discussing the subject of this address, turned to me with the quick remark, "What I can not understand is why you, a scientist, should pay any attention to the attitude toward evolution of Mr. Bryan or any other layman." Others may be raising the same question, and an *apologia pro argumento meo* may be in order.

If I were only a scientist, I think I should pay no attention, beyond a smile, to writings like those of Mr. Bryan on evolution. Scientifically, it is of little moment whether Mr. Bryan or any other individual does or does not believe in evolution or in any other scientific theory.

But I am not merely a scientist; in common with the majority of the members of Section F, I am a teacher. As teachers, we may well be jealous of that freedom of investigation and freedom of teaching through which the intellectual progress of the past has been won and through which the intellectual progress of the future must come. There must, of course, be limits to this freedom—liberty must not become license; but undue restriction can lead only to mental stagnation. Mr. Bryan's proposition to delegate to state legislature or church council the determination of the orthodoxy of scientific theory savors of the Middle Ages rather than of twentieth century America. And Mr. Bryan wields an influence not to be ignored. Tremendous moral earnestness and extraordinary oratorical power make a combination potent for right, but equally potent for error if misdirected—in no case to be disregarded.

To most of us the matter may have no personal bearing; to others the crisis is immediate. The par-

[1] Address of the vice-president and chairman of Section F—Zoology—American Association for the Advancement of Science, Washington, D. C., December, 1924. This criticism of Mr. Bryan's method is based on a rather careful study of the following authentic publications: "The Bible and Its Enemies," 1921; "In His Image," 1922; "God and Evolution," in New York *Times*, February 26, 1922; "Moses *vs.* Darwin," in *Homiletic Review*, June, 1922; "Orthodox Christianity *versus* Modernism," 1923; "Is the Bible True?" in *The Bridal Call Foursquare*, November, 1924. No attention is paid to unconfirmed newspaper reports of his multitudinous speeches. References to Darwin and Huxley are based upon the following editions: "Life and Letters of Charles Darwin," Appleton, 1901; "More Letters of Charles Darwin," Appleton, 1903; "Darwiniana," Volume 2 of Huxley's Collected Essays, Appleton, 1893.

ticularly picturesque attack in the Kentucky legislature was lost by a single vote. Some states have already passed more or less extensive restrictive laws; the question is now pending in other states; and Mr. Bryan promises that the campaign is to be carried into every state legislature.

In some church colleges the crisis is also acute. Permit me to quote, without names, a letter received by the president of my own institution within the current college year:

> Do your professors present the facts of evolution to students in lectures? Do they use text-books which have the theory of evolution in them?
>
> If so, do you regard this policy injurious to the esteem in which the Bible is held by students? Are students more or less Christian on account of such tuition?
>
> In what sense, if any, could evolution and the Bible conflict?
>
> Ludicrous as these questions appear in your environment, I hope you will render me the service which the replies to these questions will bring. We are having down south here a rather heated fight among the different branches of the ——— church. The fight is very hot in ——— [name of state], and especially at ——— University.

As educators it behooves us to take notice of the trend of events and not to sit in smug security.

But, in common with many members of this section, I am not simply a scientist and a teacher but also a Christian. I recognize and respect the various shades of belief and unbelief represented in this company. The scientific spirit, which recognizes the fallibility of all belief, should exclude the spirit of dogmatism and intolerance toward honest differences in belief. You will not all agree with me, but I hope you will respect my position when I say that it is precisely as a Christian that I most resent the attitude of Mr. Bryan.

Mr. Bryan's scientific belief and religious belief are matters personal to Mr. Bryan; but when Mr. Bryan uses his moral earnestness and his oratorical genius to proclaim to the world that belief in evolution precludes belief in God or, at least, is seriously hostile to religious belief, he becomes fundamentally dangerous, not to science, but to religion. Evolution is the universal belief of science to-day; and modern youth in America is essentially scientific. Confronted with Mr. Bryan's alternative, some young men will give up science; this is unfortunate, but relatively unimportant. Others (more, I believe) will feel themselves compelled to give up religion; this I regard as an inestimable loss to them and to the Christian church. In this day of the world's desperate need of religion, I can not look with equanimity upon any movement which tends to split the forces of the church rather than to bring them into harmony, or upon any attempt to read essentially religious men out of the church because of non-essential differences in scientific or theological belief.

For these reasons I have decided, albeit with some misgivings, to attempt to discuss one phase of this age-old conflict which has been fanned into new flame by the oratory of Mr. Bryan. I shall not attempt a comprehensive defense of evolution nor a systematic harmonizing of evolution and religion; I ask your attention only to the comparison of the methods used by Mr. Darwin and Mr. Bryan in reaching their conclusions and in expounding their views. For an exhaustive treatment of this one phase of the question I have neither the time nor the requisite philosophical training. I can only hope to bring together some interesting and useful items, many of which are familiar to you and all of which are within easy reach. In the snowball fights of our boyhood the snow was available to all alike; but it was found good military tactics to delegate certain individuals to manufacture snowballs for the use of those on the firing line. Similarly, it is my hope to be able to collect material and to shape some scientific snowballs which I trust others may be able to use to good purpose.

During the last few years Mr. Bryan has been repeating to the world in most categoric form that the work of Darwin is mere guessing. Now emphatic repetition may have a marked psychological effect alike upon the speaker and the hearers, as suggested by the health formula of Coué and the organized cheering of the stadium and the all-too-familiar "pep rally." "Hypothesis equals guess" has a catchy sound; moreover, it has a considerable element of truth, particularly if "to guess" be interpreted according to the second definition of Webster (following New England dialect, but with the added authority of Milton and Dryden) as "to judge or form an opinion of, from reasons that seem preponderating, but are not decisive." In passing, it may be noted that the synonymy of the "guess" with the scientific hypothesis is not new with Mr. Bryan; the same parallel was pointed out by Huxley years ago, with the pungent comment—"The guess of the fool will be folly, while the guess of the wise man will contain wisdom."[2] The hypothesis (or "guess") has its place in Mr. Darwin's work, as in all scientific method.

In his volume entitled "The Method of Darwin," Frank Cramer has given an interesting and illuminating analysis of Darwin's work as a conspicuous illustration of scientific method. The inductive method of science includes the use of hypothesis, but does not stop there—except in the judgment of Mr. Bryan. The initial hypothesis may be the result of

[2] "Darwiniana," p. 375.

a long and laborious collection of individual facts, their careful comparison and the selection of the elements common to the series—an induction in the narrower sense of the word; it may come as a flash of inspiration with few data as a foundation—a happy "guess"; or, lastly, an old but unsupported hypothesis may be adopted and rehabilitated. However derived, the hypothesis is only the hypothesis—perhaps that of wisdom, perhaps of folly.

In the case of evolution Darwin found the rival hypotheses of creation and derivation already in the field; but the evolution hypothesis, as worked out, for example, by his own grandfather or by Lamarck, appealed to him but little at the beginning of his scientific career. In common with his scientific friends, he was a strict creationist. But the *Beagle* voyage brought the young Darwin in contact with a wealth of new facts; and to Darwin a new fact was a new starting point for hypothesis—"I can not resist forming one on every subject."[3]

Evolution was still hypothesis to him, but becoming ever more attractive. Instead of accepting it and dogmatizing, he opened that "first note-book" in 1837, which, with its successors, was to shake the thought of the world. And note the direction taken in this investigation—not a hit-or-miss collection of data, but an instinctive dash at the crux of the matter. Animals and plants under domestication are particularly subject to variation; here, if anywhere, might suggestions be expected as to the character and cause of variation in nature. Fifteen months of careful collection of data concerning domesticated animals and plants, and the accidental reading of Malthus brought him to a new hypothesis—not an alternative nor an equivalent, but a supplementary hypothesis concerning the method of evolution. Evidently a reasonable explanation of the method of evolution would make more probable the original hypothesis of evolution; historically it was this hypothesis of natural selection which brought evolution into the forefront of scientific and popular discussion.

Again another man might have stopped and dogmatized—not Darwin. It may fairly be said that the balance of his life was given to the verification of these two hypotheses. In part the verification consisted simply in the collection and correlation of more data similar to those already gathered; but, in greater degree, it included the carrying out of one deduction after another from his theory—not as ends in themselves but for verification of the main thesis. Assuming the truth of derivative origin, what was to be expected in the geological succession of the fossils? Then to the rocks for corroboration or contradiction. On the basis of evolution, what was to be expected in the relations of the faunas of Europe, North America, Africa, Australia, the Galapagos? And, again, to the maps, the museums and the journals of explorers for corroboration or contradiction. One after another the most diverse series of data were found, with singular uniformity, to confirm the main hypothesis; and thus was gradually built up that structure of interlocking hypothesis and verification which convinced Darwin himself, made speedy converts of Huxley, Lyell, Hooker, Gray and others of his intimate friends, threw the thought of the third quarter of the nineteenth century into turmoil and to-day dominates the whole scientific world.

It is to the evolution theory in general, not the theory of natural selection, that I refer as dominating the scientific world. In the nature of the case, the inductive method (the method of everyday life as well as of science) can never arrive at a demonstration; it must always remain a matter of less or greater probability. As the testing of a hypothesis brings one and another line of seemingly unrelated data into harmony, its probability increases to that of a theory; the more numerous and the more diverse the correlated data, the greater the probability of the theory, until, finally, practical certainty is attained. In the almost unanimous judgment of biologists the evolution theory has reached this status.

Of the two subsidiary theories to which the name "Darwinism" or "Darwinian theory" is properly restricted, natural selection is accepted by most biologists, although often with decided restrictions, while sexual selection is relegated by many to the rank of a somewhat doubtful hypothesis rather than theory. Note carefully, however, that the evolution theory stands upon its own evidence, independent of the subsidiary theories. Like scaffolding, useful in the work of construction but unnecessary to the permanent edifice, natural selection and sexual selection might conceivably be torn down without materially affecting the evolution theory. Parenthetically, I may add my personal conviction that the destruction of the theory of natural selection is hardly less improbable than that of the general evolution theory.

I have laid emphasis upon these familiar distinctions because they are largely ignored by Mr. Bryan. In his writings he refers indiscriminately to evolution in general, human evolution and Darwinism; in at least one passage he explicitly states that he has "used 'evolution' and 'Darwinism' as synonymous terms."[4] If I understand him correctly, Mr. Bryan's objection is not to Darwinism in its technical meaning, but to evolution in any form as applied to man. Evolution of the lower animals and of plants interests him only "as the acceptance of an unsupported hy-

[3] "Life and Letters," Vol. 1, p. 83.

[4] "Orthodox Christianity *versus* Modernism," p. 35.

pothesis as to these would be used to support a similar hypothesis as to man."[5] Human evolution and the evolution of the lower forms rest upon similar evidence; and Mr. Bryan denies one as categorically as the other. I believe I am doing him no injustice in limiting my further discussion to evolution in its general sense, applying alike to man and the lower forms of life, but independent of any theories as to its method.

Mr. Bryan outlines the matter as follows:

> The issue can be presented in two questions: First, is Darwin's hypothesis (evolution applied to man) true or false? Second, if false, is it harmful?[6]

Granting, in common with theistic evolutionists, that God "could make man by the long-drawn-out process called evolution just as easily as he could make him by separate act," Mr. Bryan continues: "The question is narrowed down to one of fact—Did God create man by evolution or by separate act?"[7]

Eliminating Mr. Bryan's restriction to man and his confusion of evolution and Darwinism, the first of his questions becomes—Is evolution true? This question Mr. Bryan answers as follows:

> In order that there may be no misunderstanding as to the position of those who believe as I do, let me say that the evidence is not sufficient to establish evolution as the process employed by the Almighty in either plant life or in animal life below man. I am aware that many scientists deal with evolution as if it were an established fact, but no one is compelled to accept any scientist as an authority except as the facts support him. The world can not be warned away from investigation by a scientific gesture. The scientist should be the last to ask that opinion be accepted as a substitute for fact.[8]

But Mr. Darwin has collected a library of facts, and it is Mr. Bryan who is doing the gesticulation, although I grant that it is hardly a "scientific gesture."

As Mr. Bryan demands facts rather than opinions, let us note briefly the main lines of evidence in favor of the evolution theory. This evidence was summarized by Huxley for the ninth edition of the Encyclopaedia Britannica in a statement which is valid today as well as in 1878. The seven categories of Huxley's summary (all of which were mapped out in Darwin's "Origin of Species") are so familiar to every biologist that they may be mentioned by title only, as the evidence from

(1) Embryology
(2) Homologies
(3) Geographical distribution
(4) Rudimentary organs
(5) Classification
(6) Modification under varying conditions
(7) Geological succession.

Let us see how Mr. Bryan meets these various lines of evidence. The numbering corresponds to that of the preceding list.

(1 to 3) To the first three of these items (embryology, homologies and geographical distribution) I find no reference in Mr. Bryan's writings, unless a mere mention of Darwin's emphasis on the similarity of human and simian embryos may be construed as a reference to the argument from embryology.

(4) To the evidence from rudimentary organs he gives hardly more attention—merely a sarcastic reference to a young collegian, whose faith is shaken when his "attention is called to a point in the ear that is like a point in the ear of the ourang, to canine teeth, to muscles like those by which a horse moves his ears."[9] This is hardly an adequate treatment of the one hundred and fifty and more rudimentary organs found in man alone.

(5) Similarity of structure as between man and the apes Mr. Bryan does admit; in fact he says that "the whole case in favor of evolution is based on physical resemblances."[10] But he evidences no appreciation of the universality of gradations in structure upon which the classification of both animals and plants depends.

(6) Mr. Bryan's discussion of modifications is rather astounding—a flat denial, on Biblical authority, that there can be such modifications. The italics of the following quotation are his own:

> Evolution joins issue with the Mosaic account of creation. God's law, as stated in Genesis, is *reproduction according to kind;* evolution implies reproduction *not* according to kind. While the process of change implied in evolution is covered up in endless eons of time it is *change* nevertheless. The Bible does not say that reproduction shall be *nearly* according to kind or *seemingly* according to kind. The statement is positive that it is *according to kind,* and that does not leave any room for the *changes* however gradual or imperceptible that are necessary to support the evolutionary hypothesis.[11]

Such changes as have been actually observed in pigeons and cabbages are calmly ignored by Mr. Bryan; he also appears oblivious of the rather palpable fact that not all the present diverse human races

[5] "God and Evolution."
[6] "Moses *vs.* Darwin," p. 446.
[7] "Moses *vs.* Darwin," p. 447.
[8] "Moses *vs.* Darwin," p. 447.
[9] "In His Image," pp. 111, 112.
[10] "Orthodox Christianity *versus* Modernism," p. 35.
[11] "In His Image," p. 104.

can be exactly like the traditional ancestors demanded by his theory.[12]

(7) Mr. Bryan's attitude on geological succession is set forth in a series of statements, not perfectly clear in all points, but apparently intended as a categoric denial of the presence of connecting forms among the fossils. The following passage is representative:

> Wherever there is found living to-day any species of which an ancestor has been found in the rocks the living descendant is like the fossil ancestor. If this is what the evidence proves, why should we assume the truth of an hypothesis which is contradicted by everything which has been found and supported by nothing?
>
> Darwin insisted that his hypothesis should be accepted even tho the missing links had not been found, and evolutionists still insist that the hypothesis should be accepted even tho the missing links have not yet been found. They boldly demand that we substitute a guess for the Word of God even tho the guess has not been proven—in fact, has been disproven by all the evidence.[13]

Mr. Bryan can see no evidence for evolution in the marvelous wealth of fossil forms, some of them obviously intermediate in character between distinct species, genera or larger groups of to-day, and others forming unbroken gradational series between earlier and later fossil forms or between fossil and recent species. Darwin, in 1859, counted the scarcity (not absence) of connecting forms the greatest objection to his theory, and met it with his characteristic frankness. Twenty-one years later conditions had so changed as to justify Huxley's exclamation:

> If the doctrine of evolution had not existed, palaeontologists must have invented it, so irresistibly is it forced upon the mind by the study of the remains of the Tertiary mammalia which have been brought to light since 1859.[14]

And H. F. Osborn expresses himself even more strongly in 1910:

> The complete geologic succession of the vast ancient life of the American continent was destined to demonstrate the evolution law.[15]

This difference in reaction of Mr. Bryan and the paleontologists to the evidence of the fossils is exactly paralleled by the difference in reaction of Mr. Bryan and biologists in general to other lines of evidence for evolution. Mr. Bryan advances no new evidence; the data collected by scientists he ignores or denies. To the biologists the evidence seems conclusive for evolution; to Mr. Bryan it has no significance. In large part, doubtless, this difference is due to Mr. Bryan's simple ignorance of the facts. Ignorance of the details of biology is no disgrace to a lawyer; but a lawyer should be slow to pronounce a judicial decision upon technical evidence which he does not understand.

In larger part, however, Mr. Bryan's hostile attitude is due to the fact that he does not approach the matter with an open mind. In theory he recognizes that the "hypotheses of scientists should be considered with an open mind. Their theories should be carefully examined and their arguments fairly weighed";[16] practically the whole matter is decided for him in advance without reference to the scientific data. "The Bible," he writes, "not only does not support Darwin's hypothesis but directly and expressly contradicts it."[17] Further, the Bible, according to Mr. Bryan, is "the revealed will of God, and therefore infallible";[18] and other statements imply very clearly that Bible interpretation must be strictly literal throughout.

Here is no scientific method in induction—hypothesis tested out by deduction and verification. Here is no question of greater or less probability; in such deductive reasoning correct logic must lead to a correct conclusion, *provided,* of course, that the first assumption is correct; evolution must be false, *provided* the first two chapters of Genesis are literal and accurate science. But what is Mr. Bryan's guarantee of the literal infallibility of the Bible? This view has not been universally held by the leaders of religious thought in past centuries; it was not accepted, for example, by Luther or Calvin, by Augustine or Jerome. Going back to New Testament times, it was a theologian, not a scientist, who warned the Corinthian Church that "the letter killeth, the spirit giveth life." And, going still a step further back, to the author of our faith, note how whole sections of the Old Testament code are amended in the brusque and authoritative formula, "It was said to those of old time . . .; but I say unto you . . ." In him the Law and the Prophets were fulfilled; but how unique and unexpected the form of the fulfilment! Jesus Christ was a Modernist, not a Fundamentalist, in the matter of Old Testament criticism; and the Gospels are full of his efforts to overcome the deadly literalism even of his own disciples. Take, for ex-

[12] For fuller discussion see Piper, C. V., "Does the Bible teach evolution?" SCIENCE, Vol. 56, p. 109, July 28, 1922.

[13] "Moses *vs.* Darwin," p. 447.

[14] "Darwiniana," p. 241.

[15] "The Age of Mammals," p. 10.

[16] "In His Image," p. 93.

[17] "God and Evolution."

[18] "Orthodox Christianity *versus* Modernism," p. 5.

ample, this dialogue from the fourth chapter of John: "I have meat to eat that ye know not. . . . Hath any man brought him aught to eat? . . . My meat is to do the will of him that sent me and to accomplish his work." Verily, "the letter killeth; the spirit giveth life." The dogma of a literally inerrant Bible is not Biblical, not Christian. It is not with the Bible, but with Mr. Bryan's interpretation of the Bible that evolution is in conflict.

What are the alternatives? Mr. Bryan says: "The Bible is either the Word of God or merely a man-made book."[19] This method of exclusion is always dangerous. Darwin applied it to the Parallel Roads of Glen Roy to his cost. "My error," he writes, "has been a good lesson to me never again to trust in science to the principle of exclusion."[20] There is very likely to be a third alternative hiding somewhere; in the interpretation of the Bible it is exactly this third alternative which is accepted by the theistic evolutionist to-day, as well as by the great majority of intelligent Bible students. To these men the Bible is not the "Word of God" in the sense of verbal dictation from God; no more is it "merely a man-made book"; but it is a progressive, evolving revelation of God's will to man, changing with the evolution of the human race. Moreover, it is a text-book in religion, not in science. A cardinal of the time of Galileo described the Bible as teaching "how to go to Heaven, not how the heavens go." Mr. Bryan writes that "it is more important that one should believe in the Rock of Ages than that he should know the age of the rocks";[21] he might well have added that it is of the "Rock of Ages" that the Bible treats—not of the "age of the rocks" nor their contained fossils. The lesson of the first chapter of Genesis is the creatorship of God, not details of the method. With Genesis, thus interpreted, evolution has no quarrel.

I do not question that Mr. Bryan is perfectly sincere in his belief in the falsity of the evolution theory and its danger to the Christian religion; but is he perfectly sincere in the character of his argumentation? Certainly he is not frank. His method is that of the lawyer striving to win his case rather than that of the earnest seeker for truth. The contrast with Darwin is most striking and not to the advantage of the professed defender of the faith of the Christ who characterized himself as "the truth."

Three short quotations show three phases of Darwin's attitude to truth:

> I believe there exists, and I feel within me, an instinct for truth . . . of something of the same nature as the instinct of virtue.[22]
>
> I have steadily endeavored to keep my mind free so as to give up any hypothesis . . . as soon as facts are shown to be opposed to it.[23]
>
> As I am writing my book [The Origin], I try to take as much pains as possible to give the strongest cases opposed to me.[24]

That he succeeded is seen in the impression made upon those who knew him best. Let Huxley speak for all:

> It has often and justly been remarked that what strikes a candid student of Mr. Darwin's works is not so much his industry, his knowledge, or even the surprising fertility of his inventive genius; but that unswerving truthfulness and honesty which never permit him to hide a weak place or gloss over a difficulty, but lead him, on all occasions, to point out the weak places in his own armour, and even sometimes, it appears to me, to make admissions against himself which are quite unnecessary. A critic who desires to attack Mr. Darwin has only to read his works with a desire to observe, not their merits, but their defects, and he will find, ready to hand, more adverse suggestions than are likely ever to have suggested themselves to his sharpness, without Mr. Darwin's self-denying aid.[25]

In Mr. Bryan's writings, on the other hand, no objections are mentioned, no difficulties suggested; instead we find the ex-cathedra statement, oft repeated, that there are no evidences for evolution, therefore no difficulties with Mr. Bryan's position. It is tempting to multiply quotations; one must suffice:

> Neither Darwin nor his supporters have been able to find a fact in the universe to support their hypothesis.[26]

And, in another connection, I have already traced out how the main lines of evidence upon which the evolution theory is based are, one after the other, simply ignored or categorically denied. Very different from the method of Darwin—rather the method of an earlier critic of whom Darwin himself writes:

> The reviewer gives no new objections, and, being hostile, passes over every single argument in favor of the doctrine. . . . As advocate, he might think himself justified in giving the argument only on one side.[27]

As an advocate, yes; as a scientist, no.

Many such omissions of relevant evidence and some

[19] "Orthodox Christianity *versus* Modernism," p. 9.
[20] "Life and Letters," Vol. 1, p. 57.
[21] "Moses *vs.* Darwin," p. 452.
[22] "More Letters," Vol. 1, p. 61.
[23] "Life and Letters," Vol. 1, p. 83.
[24] "More Letters," Vol. 1, p. 95.
[25] "Darwiniana," p. 184.
[26] "God and Evolution."
[27] "Life and Letters," Vol. 2, pp. 19, 24.

apparent misrepresentations of the evolution theory and its supporters may well be due to Mr. Bryan's unfamiliarity with the facts in the case. A lawyer who does not know law or a doctor who has not studied medicine is a quack and subject to legal control; even a school teacher must be duly certificated. Is there not a moral obligation that a man professing authoritative leadership on evolution should first familiarize himself with the subject?

In some cases it is difficult to believe that Mr. Bryan's omissions are due to ignorance. Mr. Bryan lays the utmost stress upon the verbal accuracy of the Biblical story of creation. But which story? We can hardly believe that his attention has never been called to the fact that there are two such stories in Genesis, the first ending with the third verse of the second chapter. Each has its great moral teaching, different from the other but consistent with it. But, literally interpreted, their mutual inconsistency is no less glaring than the inconsistency of either, thus interpreted, with the observed facts of geological succession. There may be a reference to this contradiction in Mr. Bryan's writings; I have never seen one. Is it conceivable that Charles Darwin would ignore a difficulty of this sort?

Contrast, again, the dogmatic certainty of Mr. Bryan concerning a subject wholly aside from his main professional work with the modesty and caution of Mr. Darwin, whose life was devoted to the study of this problem. A review in a prominent and rather conservative church paper contains these words:

> There is something interesting in the naïve notion which Mr. Bryan has of the contrast between the absolute certainty of his own religious opinions and the merely probable opinions of scientific men. He refers to the fact that Darwin is continually using such words and phrases as "apparently," "probably," "we may well suppose." "The eminent scientist," says Mr. Bryan, "is guessing." Because Darwin and other scientific men, in the truly scientific spirit, recognize their beliefs as only more or less probable, and claim for them no absolute certitude, Mr. Bryan considers that their opinions are of no consequence at all. He *knows*. For him it is a matter of absolute certainty that there is a God, that every sentence of the Bible is the word of God, and that he himself understands aright every sentence of the Bible. All his religious opinions are utterly above the realm of probability, dwelling in a serene and heavenly atmosphere of absolute certitude.[28]

No less certain is he concerning scientific matters. Let me remind you of a sentence already quoted—"*Let me say* [the italics are mine] that the evidence is not sufficient to establish evolution as the process employed by the Almighty, etc." Darwin gave twenty years to the collection of material for "The Origin of Species" and "thirteen months and ten days' hard labor" to the preparation of the manuscript.[29] Perhaps a similar application to the subject would leave Mr. Bryan, however he might decide the main issue, less sure that he had probed the problem to its utmost depths. Perhaps it would bring him to an appreciation of the meaning of "probably" in scientific argument.

Mr. Bryan's writings are done in a style which can hardly be characterized as calmly scientific. He expresses regret at the "epithets" with which the "liberals" attempt to "terrorize the masses of the church into accepting without proof or even discussion the views of those who put their own authority above the authority of the Bible."[30] But his own constant play on the word "guess," his repeated sarcastic parody of the evolution of eyes from "freckles" and of legs from "warts" are hardly conducive to calm discussion; his aphorism that "cousin ape is as objectionable as grandpa ape,"[31] and his statement that evolution gives Christ "an ape for his ancestor on his mother's side at least,"[32] are suggestive of the famous speech of Bishop Wilberforce in 1860, perhaps also deserving of a reply like that of Huxley on that historic occasion. Such rhetoric is entertaining, and, in this day of slogans, may be effective with the masses—perhaps also with state legislatures; but it is not science; nor is it the method of Darwin. Sarcasm and ridicule are as conspicuous for their absence from Darwin's writings as for their presence in Bryan's.

In addition to the question of the truth of the evolution theory, Mr. Bryan raises the second question of its harmfulness. By his own formulation—"If false, is it harmful?"—this question becomes relevant only in case evolution is proven false. Although by no means granting the falsity of evolution, I wish to call your attention briefly to two points in Mr. Bryan's argument concerning its supposed harmfulness.

First, he argues, evolution, if it does not crowd God out of his universe, at least pushes him so far away in space or time as to make him negligible. I quote:

> Why should we want to imprison such a God in an impenetrable past? This is a living world; why not a *living* God upon the throne? Why not allow him to work *now*?[33]

[28] Wm. North Rice, in *The Christian Advocate*, April 20, 1922, p. 478.

[29] "Life and Letters," Vol. 1, p. 70.

[30] "Orthodox Christianity *versus* Modernism," p. 15.

[31] "In His Image," p. 102.

[32] "The Bible and Its Enemies," p. 35.

[33] "In His Image," p. 106.

Is not this exactly the position of the theistic evolutionist, for whom natural law is merely a human attempt to formulate the method of divine activity, and evolution a human attempt to formulate the method of divine creation?

But not all evolutionists are theists; and Mr. Bryan urges that it is evolution which has made them agnostic or atheistic. As his principal illustration he uses the familiar case of Darwin—a gradual drift from an orthodox belief to a condition of agnosticism, albeit with times, even in his later life, when he felt himself "compelled to look to a First Cause having an intelligent mind in some degree analogous to that of man," and in which he deserved "to be called a theist."[34] But is it so certain that evolution was the sole cause or even the chief cause of Darwin's change of belief? Other elements should certainly be considered.

First among these is the matter of continued ill health. When one considers the mass of scientific work accomplished, in connection with the bodily weakness which reduced the working day to a minimum and necessitated frequent periods of complete rest and sanitarium treatment, can one wonder that, in his own words, his mind should become a "kind of machine for grinding general laws out of large collections of facts," and that there should be a corresponding "atrophy of that part of the brain . . . on which the higher tastes depend?"[35] All are familiar with the pictures of the boy Darwin reading Shakespeare in the old window of the school, of the young traveller carrying Milton's "Paradise Lost" on shore trips in South America when only one volume was possible, and the aged scientist realizing with regret that he could no longer "endure to read a line of poetry" and that Shakespeare had become "so intolerably dull that it nauseated" him.[36] May it not well be that his loss of formal religious faith was a parallel of this atrophy of the esthetic sense, seen also in the partial loss of the love for music, art, and, in lesser degree, for natural scenery. It may well be questioned, however, whether Darwin's scientific caution and questioning attitude did not lead him to an over-emphasis of his religious doubts, particularly in reaction against the dogmatic certainty of many of his critics.

There is another element in this problem to which Bryan has not referred. May not the responsibility for Darwin's loss of religious belief be laid, in part at least, upon the impossible character of the dominant orthodox theology of his day. In the storm of invective which burst upon his head after the publication of the "Origin," is it strange that even a man of Darwin's amazing charity and poise should have turned away from organized religion as well as dogmatic theology?

But neither in 1859 nor in 1924 can the blame for the conflict of evolution and religion be placed wholly on the theologians. There is an *odium scientificum* as well as an *odium theologicum*. In 1859 there were materialistic scientists who seized eagerly upon the evolution theory as a new weapon for attacking Christian faith; among the theologians, on the other hand, were strong men who, from the start, recognized the truth of evolution as an aid to faith. Today, again, very many leading theologians take issue with Mr. Bryan's position as sharply as can the scientist; and some biologists are hardly less dogmatic in their support of a materialistic philosophy than is Mr. Bryan in his attack upon evolution. It may fairly be questioned whether the materialistic scientist is not as responsible for the present anti-evolution flareup as is Mr. Bryan himself. It is unfortunate that Mr. Bryan could not have directed his campaign against this materialism of individual evolutionists rather than against the evolution theory itself.

I have tried in this address to emphasize the hopeless inadequacy of the method exhibited in Mr. Bryan's attack upon the evolution theory, and the illegitimacy of his claim to popular leadership in such an issue. For his religious earnestness and his devotion to moral reform I have profound respect, although I deeply regret the reopening of the age-old conflict of science and religion under his leadership.

From the present phase of this unhappy conflict, two happy results are, however, already becoming apparent. On the one hand, there is an increasing popular interest in evolution and a more intelligent understanding of its significance. On the other hand, an increasing number of our leading scientists are publicly proclaiming their own theistic philosophy, and emphasizing anew the essential harmony of a progressive scientific belief with real religion. I rejoice in the public utterances of such men as Conklin, Coulter, Millikan and Osborn. May their tribe increase! And may their efforts combine with the increasing popular interest in science toward the bringing in of the day when a more scientific religion and a more religious science shall join in a common welcome to truth, whether revealed in nature, in human life, or in the Bible, and shall present an unbroken front in the struggle for the higher evolution of the human race.

Ohio Wesleyan University Edward L. Rice
Delaware, Ohio

[34] "Life and Letters," Vol. 1, p. 282.
[35] "Life and Letters," Vol. 1, p. 81.
[36] "Life and Letters," Vol. 1, p. 81.

WILLIAM FRANCIS HILLEBRAND

DR. WILLIAM F. HILLEBRAND, chief chemist of the United States Bureau of Standards, died on the afternoon of February 7, 1925. The immediate cause of his death was heart failure, the remote result of a serious case of pneumonia from which he suffered several years ago. His loss is most keenly felt by all his associates at the Bureau of Standards and his many friends in Washington. He leaves a gap in the ranks of chemists that will not soon be filled. He is survived by his widow and his two sons, William Arthur, electrical engineer of Palo Alto, California, and Harold Newcomb, professor of English in the University of Illinois.

Dr. Hillebrand was born in Honolulu, December 12, 1853. His father was not only a physician but a distinguished authority on the botany of the Sandwich Islands. The old home grounds, a veritable botanical garden, is to this day one of the show places of Honolulu. These surroundings of his earliest days, with visits to China, Java and India, and his life for a time in California implanted in him a deep interest in nature which he kept to the last. After he became chief chemist of the Bureau of Standards he moved to Cleveland Park, a suburb half a mile from the laboratories. There he had a garden in which he experienced the usual pleasures and disappointments of those who delve in the soil. As long as his eyesight permitted he frequently carried a bird book on his way through the woods to and from the bureau. He was an ardent angler for small-mouthed black bass, but took little interest in other fishing, at least in his later years. He was evidently much chagrined in the summer of 1923, but said little about it, to find that he was unequal to the exertion of trout fishing in Estes Park, Colorado.

Among his diversions were an afternoon at a baseball game or an evening playing skat. He enjoyed good music, and evening visitors at his home would sometimes find him seated at the piano. Philately was a hobby he gave up in his later years. One of his greatest pleasures was reading all manner of books. Only a month before his death he read for the first time Needler's translation of the Nibelungenlied, his first real acquaintance with that classic.

Strange to say, Dr. Hillebrand did not at an early age display any inclination toward the study of chemistry. Not until 1872, after two years at Cornell University, did he decide to become a chemist, and then only because it was suggested to him as a subject of interest and importance. Accordingly, in the fall of that year he entered the University of Heidelberg, where he studied chiefly under Bunsen and Kirchhoff. While there he and T. H. Norton, working together, were the first to prepare metallic cerium, lanthanum and the mixture then called didymium. Their joint paper appeared in Poggendorff's Annalen in 1875, in which year Hillebrand attained his doctorate. For another year he remained at Heidelberg to continue his work on these metals. Until that time they had been considered as divalent and closely related to calcium. Dr. Hillebrand's determinations of their specific heats proved that they are trivalent, and therefore rare earth metals. It may be noted incidentally that he discovered the pyrophoric property of cerium filings. Many years later cerium-iron alloy came into commercial use for the tips of gas lighters.

The scholastic year 1876–77 was spent with Fittig at the University of Strassburg. Their joint paper on quinic acid represents his only serious divagation into the field of organic chemistry. One other paper written at Strassburg was on the crystal form of the ester of tetraacetylquinic acid.

The next winter, 1877–78, was spent at the Mining Academy at Freiberg. Although he published nothing from that institution, he there learned methods of assaying and no doubt received the impulse which started him along the path he ever afterwards followed.

Returning to America, Dr. Hillebrand worked for a time as an assayer in Leadville, Colorado, but soon seized the opportunity of an appointment on the staff of the United States Geological Survey. From 1880 to 1885 he was stationed at Denver, where there was no dearth of minerals to arouse his interest and upon which to exercise his growing skill as an analyst. While in Denver he wrote six papers, all on minerals and all but two in joint authorship with Whitman Cross. After his transfer to Washington this work continued. It did not cease when he became chief chemist of the Bureau of Standards in 1908, for he found time to work in the laboratory and to write several papers. In all he wrote about one hundred papers.

Once while chief chemist Dr. Hillebrand somewhat diffidently remarked that he had been appointed to the position because he had some reputation as an accurate analyst. That was his excuse for insisting that his subordinates should take the utmost pains in their work and not let themselves fall into careless ways. Dr. Hillebrand was too modest, because he will always stand as the first chemist who thought it worth while to make the most accurate possible analyses of rocks and minerals, to actually determine all their constituents, to have in the tabulation of his analyses no item large or small marked "undetermined," to report "traces" only when they were really such. The extra time devoted to his accurate and

detailed analyses was well spent, because the results revealed to geologists unexpected resemblances or differences in rocks. For instance he found that the igneous rocks of the Rocky Mountain region contain, on the average, much greater percentages of strontium and barium than the rocks in the eastern or the farther western parts of the country. If the reason for this fact is now unknown, some day it will throw a flood of light upon some geologic generalization.

Dr. Hillebrand's failure to identify helium in the gases that are evolved from uraninite by the action of hydrochloric acid was no doubt a bitter disappointment to him, though he dwelt upon it lightly. In 1890 he treated powdered uraninite in a test-tube with hydrochloric acid and observed the slow, continuous evolution of tiny bubbles, which he collected and analyzed. The gas was shown not to be carbon dioxide or hydrogen sulphide, and he succeeded in converting all but a small residue into ammonia. He felt justified in the belief that this residue was nitrogen that had not reacted. Years later his two papers on the occurrence of nitrogen in uraninite were called to the attention of Ramsay, who soon afterwards announced his discovery of helium.

Another matter from which Dr. Hillebrand received no benefit and little credit may be mentioned. In 1904 he called attention to the enormous quantities of potash that are volatilized and lost during the clinkering of Portland cement, and suggested that it should be collected and utilized. The recovery of this potash is now a commercial process.

Because he had no real forerunner in mineral and rock analysis, Dr. Hillebrand perforce had to devise general procedures suitable for the different types and special methods for the determination of individual elements. As his work became known through his published papers, there came a demand for some description of his methods. Perhaps he brought the demand upon himself with greater force by publishing in 1894 "a plea for greater completeness in chemical rock analysis." He met the demand in 1897 by writing a fifty-page section of Bulletin 148 of the Geological Survey. This was soon translated and published in Germany. It dealt only with the silicate rocks. Bulletin 176, of 114 pages, appeared in 1900. The carbonate rocks appeared in the title of Bulletin 305, in 1907, and thereafter. This bulletin was also translated into German. In 1910 appeared Bulletin 422, which was once revised. The series of bulletins, each an improvement upon its predecessors, culminated with the publication in 1919 of No. 700, a book of 285 pages. Papers on the determination of various elements appeared in the intervals between his bulletins. No chemist who consults any of these publications can fail to have impressed upon him the necessity of taking infinite pains at every step, if he is to make a really accurate analysis.

For a year prior to his death Dr. Hillebrand had devoted nearly all his time and energy to the preparation of a book on inorganic analysis. Ten years earlier he had been advised to write it, but had hesitated to undertake the task. It is fortunate that his notes for the projected book are so complete that his collaborator, G. E. F. Lundell, will be able to finish it.

In 1908 Dr. Hillebrand became the second chief chemist of the Bureau of Standards, in which position he remained until his death. In those early days of the bureau, which was just beginning to be well and favorably known throughout the country, he had only a handful of younger men under him. He was able to spend much time in the laboratory and wrote several papers on the composition of different minerals or on analytical methods. During this time he prepared his last two bulletins for the Geological Survey. As the bureau grew in size and importance the demands upon his time and energy increased, so that he entered the laboratory less and less frequently. He did not complain about this, but no doubt he many a time wished he were at his laboratory bench.

In 1908 the bureau had a modest list of standard analyzed samples: three of iron taken over from the American Foundrymen's Association, an argillaceous limestone, a zinc ore. These carefully prepared materials, the composition of which was known with the closest possible approach to exactness, were too near to his chosen field for him to neglect or ignore, particularly because they were intended to serve as standards for checking analytical methods. In the sixteen years that followed he took the greatest interest in the standard samples and did everything in his power to increase their number and usefulness, so that the bureau now distributes annually about five thousand samples representing sixty-five materials. He always jealously guarded the integrity of these samples and would not countenance including among their number any materials the composition of which was not known with an accuracy great enough to satisfy him.

He was a kindly chief, ready to discuss the problems and worries of his subordinates, not given to making the facile excuse that he was too busy to talk. He gave to every man his due of credit, he sought and obtained promotions in rank and salary for those who in his opinion were deserving. Always modest about his own attainments, he gave his associates full credit for whatever the chemistry division of the bureau accomplished. He took his administrative duties seriously and suffered undue worry lest

he should be found wanting. A man in his position must many times make decisions relating to the use of government funds and be tempted to divert them from their specifically authorized use to some other he may think more worthy. Dr. Hillebrand "leaned backward" in his uprightness and would not countenance any violation of the letter or the spirit of the law. His honesty in these matters was but a further expression of the integrity of purpose that was the great guiding principle of his life, the principle that made him perform a routine analysis with the greatest care, that made him give to any task the best that was in him.

A man of such character and attainments can not avoid membership in scientific societies or escape honorary recognition by the scientific world. Dr. Hillebrand was a fellow of the American Association for the Advancement of Science, a member of the American Chemical Society, of the American Society for Testing Materials, the Washington Academy of Sciences, the Geological Society of Washington and other societies. His election to membership in the American Philosophical Society and in the National Academy of Sciences must be regarded as honors. So, too, was the award of the Chandler Medal by Columbia University in 1916. His address on that occasion, "Our analytical chemistry and its future," is well worth reading by any chemist who may be disposed to regard analytical work as uninteresting and not worthy of a man's best efforts.

The American Chemical Society made great use of Dr. Hillebrand's ability and prestige as a chemist. For years he was an associate editor of the Journal, and with conscientious care read and criticized papers that were submitted to him for his opinion. His criticisms were constructive, and when adverse comment seemed called for he was tactful and considerate of the feelings of the authors. Later he acted in the same editorial capacity for the *Journal of Industrial and Engineering Chemistry*. During the first years of *Chemical Abstracts* his abstracts were sent in promptly and they were so carefully prepared as to need no editing.

He served on the society's committees, particularly those that had to do with analytical methods. Until the beginning of this year he was chairman of the Supervisory Committee on Standard Methods of Analysis. No division or section of the society may publish any analytical procedure as a standard without the formal approval of this committee. As its leader Dr. Hillebrand had now and then to bear the brunt of criticism that verged on harshness. He did not let this divert him from the stand he took when he was sure that he was right. On the other hand he was not obstinately dictatorial but would patiently listen to argument, willing to be convinced.

In 1906 he was president of the American Chemical Society. The address he delivered on retiring is one that is worth reading, especially with the thought in mind that he wrote it after a strenuous year of coping with internal dissensions that threatened to disrupt the society. Some of the members most concerned with industrial chemistry felt that their interests were being slighted in the journal, and there was danger that they would form an independent society. The prompt, energetic and tactful action of the leaders in the society, with Dr. Hillebrand at their head, averted this danger by the establishment of the *Journal of Industrial and Engineering Chemistry*.

With this inadequate and all too brief sketch of Dr. Hillebrand we close. We could not let him pass without a word, but no word can express how we feel about him. No trite and hackneyed phrases can make a stranger know him as he was, or brighten his memory for his friends.

> Praise from a friend, or censure from a foe,
> Are lost on hearers that our merits know.

C. E. WATERS

BUREAU OF STANDARDS
WASHINGTON, D. C.

SCIENTIFIC EVENTS

THE DRIFT OF THE "MAUD"[1]

THE following note is based upon wireless messages which have appeared during the last three years in the London *Times* reporting the progress of Amundsen's vessel, the *Maud*, in the Arctic. The object of the expedition was to drift in the ice across the North Polar Basin from the coast of Siberia. Amundsen made the northeast passage in 1918–20, but instead of beginning his drift at once, he was compelled to put into Nome, Alaska, in July, 1920. After various delays, the *Maud* finally sailed from Point Hope on July 26, 1922, but two days later Amundsen left the ship for Point Barrow, to make his unsuccessful attempt to fly to the Pole, and the voyage was continued under the command of Captain Wisting. Herald Island was sighted on August 7, and on August 22 the vessel was frozen in. In a wireless message her position on December 15 was given as lat. 73° 20′ N. and long. 173° W. (an error for 173° E.), and she began to drift slowly to the northwest, her position on March 10, 1923 being lat. 74° 2′ N., long. 170° 20′ E. In an undated report sent out towards the end of June it was given as 75° 25′ N., and 165° E. This

[1] From the *Geographical Journal*.

course was likely to take the *Maud* well north of the New Siberian Islands. Up to August it had coincided fairly closely with the drift of the *Jeannette* under De Long, 1879–81. Had the *Maud* continued to the northwest, past the De Long Islands, she would ultimately have followed the approximate course of the *Fram,* which in 1894 had been frozen in to the west of the New Siberian Islands; but during September and October a strong "northwest" (? northeast) gale of long duration was encountered which carried her more to the south and west. Thus on September 6, 1923, the position of the *Maud* was 76° 16′ N. and 163° 30′ E., and towards the end of October it was 75° 10′ N. and 159° 30′ E. She was now drifting closer to the New Siberian Islands (on December 18 her position was 75° 14′ N. and 158° 46′ E.), and this may have opened the pack. The last message received from Captain Wisting stated that the *Maud* was clear of the ice on August 9, 1924. This probably means she was then in open water, and had been navigating in the ice for perhaps a month previously. Her position on that date was 76° 25′ N. and 143° 20′ E. The next position given was that for August 27, when she is described as having passed Laptev Strait, and being then 7 miles from Cape Baranov, "having been compelled, from the impossibility of rounding the New Siberian Island, to turn and go west of Kotelni Island," the most westerly of the group. The attempt to drift across the North Polar Basin had therefore been abandoned, and the remainder of the message makes it clear that Captain Wisting was attempting to return to Bering Strait.

Cape Baranov is in lat. 69° 40′ N. and long. 164° E.; the *Maud* was evidently trying to sail past it eastwards, but ice probably pressed against the coast and no passage was possible. She then turned north towards the Bear Islands, off the mouth of the Kolyma. The position of the *Maud,* when the final message was despatched on November 9, was given as 4 miles north of Four Columns Island. This must be the Chetyirekh-Stolbovoi Island (Lighthouse Island) of the Admiralty Chart, in 70° 40′ N. and 162° E. (In Nordenskiöld's "Voyage of the *Vega,*" vol. 1, p. 428 (English transl.) a reference is made to four columns of rock on Lighthouse Island.) The *Maud* has therefore failed to reach Bering Strait, and is probably once more frozen in near Lighthouse Island. Captain Wisting says that the *Maud,* which had on more than one occasion been subjected to heavy ice pressure, had sprung a small leak, and also that there was only sufficient motor oil fuel left for a day and a half. An aeroplane was carried on the *Maud,* but little success attended its flights. Trials were made on June 5 and 12, 1923, but on June 22 it was damaged in attempting to take off, and on July 16 it was completely wrecked through a forced landing. Scientific observations have been carried out continuously and successfully, particularly with reference to the tidal currents.

THE BALTIMORE MEETING OF THE AMERICAN CHEMICAL SOCIETY

THE sixty-ninth meeting of the American Chemical Society will be held in Baltimore from April 6 to 10. The first day will be devoted to registration and council meetings. On the morning of the second day a general meeting will be held which will be addressed by the Governor of Maryland, the Mayor of Baltimore, Dr. James F. Norris, president of the society, and Dr. Neil Gordon, chairman of the Maryland section of the society. The following days will be devoted to divisional meetings which will be held in the chemistry building at the Johns Hopkins University. There will be several excursions and a number of social features are planned.

All divisions and sections, except the fertilizer division, will meet at Baltimore. The divisions of industrial and engineering chemistry, physical and inorganic chemistry, organic chemistry, and chemical education will hold special general programs on Tuesday afternoon in halls adjacent to hotel headquarters.

In these general sessions the program of the division of industrial and engineering chemistry will consist of general papers, such as "The future of industrial synthetic organic chemicals in the United States," by Charles H. Herty; "Chemistry and the leather industry," by Allen Rogers; "A quarter of a century of chemistry in rubber," by W. C. Geer. Arrangements for full discussion of these papers are being made.

The division of organic chemistry has invited the biological, cellulose, dye, medicinal products, and sugar divisions to join with it in a general program. The paper offered by the division of chemistry of medicinal products by Dr. Edwin C. White, of Baltimore, on "Dyes used as tests of liver function," will be presented at this time. Additional papers will be announced later.

The division of chemical education offers for the general program a symposium on "The place of the electron in the teaching of chemistry," R. A. Baker presiding.

The petroleum division will participate in the symposium on corrosion with the industrial division and will present a list of papers on petroleum technology A dinner for the division is announced for 6 o'clock on Thursday evening.

The sugar division will specialize in papers particularly affecting the refining industry. This is especially desirable, as one of the largest sugar refineries in the country is situated in Baltimore.

The industrial division offers a symposium on corrosion, with Robert J. McKay as chairman, which will begin Wednesday morning. Such general fields as the corrosion of brasses; the corrosion of iron; the corrosion of aluminium alloys; the corrosion of stainless steel; the effect of minute films on corrosion; the corrosion of alloys of high temperatures; corrosion of antique bronzes; and the newer electrolytic theory of corrosion will be discussed. In addition, the program will contain also the names of W. R. Whitney, an exponent of the electrochemical theory; W. D. Bancroft, a recognized authority on the physical chemistry of corrosion; W. H. Bassett, an authority on corrosion resisting alloys; Guy D. Bengough and Ulick R. Evans, among the foremost authorities of England.

The paint and varnish section will hold its meetings on Wednesday and Thursday morning, with a dinner on Thursday evening. The gas and fuel section, besides a "Symposium on flames, their chemistry and controlling factors," with Professor R. T. Haslam as chairman, will have numerous original papers on gas and fuel subjects. The rubber division will arrange a symposium on either the "Measurement of the plasticity of rubber" or on the "Artificial aging of rubber by oxidation." The secretary urges all members of the society who are prepared to contribute on either of these subjects to communicate with him at once. The division will hold a dinner and smoker at 6 o'clock Thursday evening. The cellulose division will hold a symposium on "Oxy and hydro cellulose and cellulose hydrates," the discussion to be opened by Jesse Minor, Harold Hibbert, and John L. Parsons.

The division of agricultural and food chemistry wishes to announce that while the division will hold its usual meeting for the presentation of papers, the special symposium on "Insecticides and fungicides" will be held at Los Angeles rather than at Baltimore. This is done to meet a special demand from the West for a discussion of this subject.

The division of chemistry of medicinal products in addition to its regular papers will hold a symposium on "Chemistry in the field of microbiology." Papers have been promised by T. B. Johnson, Carl Voegtlin and John Churchman. One other paper will probably complete the morning program, as it is desired to give from thirty to forty minutes to each paper with time for discussion.

The division of chemical education in addition to the general program, on Tuesday afternoon will have papers of especial interest to college teachers on Wednesday morning and papers of especial interest to high school teachers on Friday morning, both under the chairmanship of Wilhelm Segerblom. Friday afternoon will be given up to additional papers, reports of committees and to a business meeting.

UNIVERSITY OF MICHIGAN BIOLOGICAL STATION

THE University of Michigan Biological Station will hold its seventeenth session for instruction and research on the shores of Douglas Lake, Cheboygan County, Michigan, from June 22nd to August 14th. Instruction in zoology will be given by Professors George R. LaRue and Paul S. Welch, University of Michigan; Dr. Chas. Creaser, College of the City of Detroit; Dr. Frank Blanchard, University of Michigan, and Professor Herbert B. Hungerford, University of Kansas; in botany by Professors John H. Ehlers and Carl L. LaRue, University of Michigan, and Professors George E. Nichols, Yale University, and Frank C. Gates, Kansas State Agricultural College.

The courses in zoology include ichthyology, limnology, entomology, ornithology, herpetology and mammalogy; while in botany the following courses are offered: Taxonomy of green cryptogams, taxonomy of the bryophytes, systematic botany, ecology, plant anatomy and plant geography. For those requiring direction in research work the following fields are suggested: The morphology, taxonomy and life histories of parasitic worms, Professor LaRue; the fishes and mammals, Dr. Creaser; aquatic insects and limnological problems, Professor Welch; birds, amphibians and reptiles, Dr. Blanchard; the aquatic hemiptera, Professor Hungerford; the bryophytes, Professor Nichols; plant physiology and ecology, Professor Gates; and taxonomy of the flowering plants, Professor Ehlers.

Under certain conditions, properly qualified graduate students may complete the requirements for the M.A. or M.S. degree by working at the station through three or four summer sessions. Inquiries should be addressed to Professor Paul S. Welch, acting director, University of Michigan, Ann Arbor, Michigan.

MEMORIAL TO DR. THOMAS L. WATSON

THE following memorial on the death of Dr. Thomas L. Watson, late state geologist of Virginia, was passed at the meting of the Association of American State Geologists held at Ithaca, New York:

During the past year one of our most beloved members, Dr. Thomas Leonard Watson, passed away, his death having occurred on November 10, 1924. His loss is most keenly felt by the association in which he was always a most active member, and he will be greatly missed by all his friends and associates in his chosen field.

To us who knew Dr. Watson best, he will be missed first as a friend, for he had the rare ability of making

lasting friendships among his associates. His official work in the State of Virginia was untiring and he represented that wonderful spirit of the New South, which put behind it all the disappointments of the Civil War, and always looked forward, devoting his energies to bringing about a renaissance of leadership and development. He was active in the founding of the present Virginia Geological Survey, and established its scientific work on a high plane as the true basis for industrial development. He was known and loved in every county of his native state and responsibility for much of its present industrial growth was rightly laid at his door.

As a teacher in the State University he upheld all its finest traditions, and added to it vision and inspiration.

Born on a plantation at Chatham, Virginia, 53 years ago, he was just in the prime of a vigorous life devoted to the upbuilding of his state along all lines. It is hard to realize that he has completed his life work, but he has set a mark for those that follow to emulate.

Be it resolved that this resolution be spread on the minutes of the association, and that copies be sent to his family and official associates in Virginia.

The Association of American State Geologists

M. M. LEIGHTON, *Secretary*

URBANA, ILL.

SCIENTIFIC NOTES AND NEWS

DR. ARTHUR R. CUSHNY, F.R.S., professor of pharmacology in the University of Edinburgh, will give the Charles E. Dohme memorial lectures for 1925 at the medical school of the Johns Hopkins University on May 8, 9 and 11. The title chosen by Professor Cushny for his three lectures is "Optical isomers in biology."

AMONG those receiving honorary degrees at the special commemorative exercises at the University of Pennsylvania on February 23 are Dr. Hubert Work, secretary of the interior, doctor of laws; Dr. Ray Lyman Wilbur, president of Stanford University, doctor of laws, and Dr. Charles Harrison Frazier, professor of surgery at the University of Pennsylvania, doctor of science.

WILLIAM H. BASSETT, metallurgist of the American Brass Company, has been awarded the James Douglas medal of the American Institute of Mining and Metallurgical Engineers, given for distinguished scientific achievement in the brass industry.

SIR HUMPHREY ROLLESTON, president of the Royal College of Physicians of London, was the recipient of the honorary fellowship of the Royal College of Physicians of Ireland, at a meeting of the college on February 7.

AT a meeting in the Pasteur Institute, December 20, the director of the institute, Dr. Roux, presented M. Paul Strauss, a member of the Academy of Medicine, with a medal for his services, as minister of hygiene, to social welfare.

PROFESSOR FULLEBORN, of the chair of tropical diseases at Hamburg, has been presented with the Japanese gold Katsurada medal.

DR. MURK JANSEN, instructor in orthopedics at Leyden, was tendered a banquet recently on his twenty-fifth professional anniversary, and substantial additions were made to the Jansen Foundation by former patients and the Netherlands Orthopedic Society.

THE REVEREND R. R. STEBBING, F.R.S., British zoologist, celebrated his ninetieth birthday on February 6.

DR. ADOLPH BARKAN, professor emeritus and one of the first members of the faculty of the Stanford University Medical School, celebrated his eightieth birthday in Rome, Italy, on January 8.

DR. VERNON KELLOGG, of the National Research Council, has been elected president of the Washington Academy of Sciences for 1925.

THE reappointment of General Amos A. Fries as chief of the Chemical Warfare Service for another term of four years has been confirmed by the United States Senate.

DR. E. D. BALL, director of scientific work in the United States Department of Agriculture, has tendered his resignation, to take effect March 4, 1925, to the incoming Secretary of Agriculture, President William M. Jardine, in order to give the new secretary the freest opportunity to select his immediate associates.

J. S. REICHERT, formerly professor of chemistry at the University of Notre Dame, has been appointed chemist in the Chemical Warfare Service, Edgewood Arsenal.

ROBERT REED, chemist for the du Pont Dye Works, has resigned, to take charge of the research work of the Lithographic Technical Foundation of the University of Cincinnati.

DR. JOHN C. MERRIAM, president of the Carnegie Institution of Washington, has left for Mexico to visit the ruins of the ancient Maya capital at Chichen Itza, Yucatan, where the institution is preparing for a thorough study of the civilization founded there. In Havana, Dr. Merriam will meet Dr. Clark Wissler, of the American Museum of Natural History, and Dr. A. V. Kidder, of Phillips Academy, members of the institution's advisory committee. The three then will sail for Progreso.

PROFESSOR E. C. JEFFREY, of Harvard University, will leave the United States for a visit to Australia and

New Zealand some time during the summer. He will devote his attention to the finding of fossil material illustrating the evolution of the Conifers of the Southern Hemisphere. During his stay in Australia, he will be the guest of the University of Sydney, New South Wales.

KNUT RASMUSSEN, the Norwegian explorer, is leaving Copenhagen for a visit to Canada. Later he will go to New York, Chicago and other cities in the United States.

DR. E. V. MCCOLLUM, of the Johns Hopkins School of Hygiene and Public Health, will give on March 16, at the American Museum of Natural History, a lecture under the auspices of the New York Academy of Sciences, on "The application of laboratory studies in nutrition to human progress."

DR. FRANCIS G. BENEDICT, of the Carnegie Nutrition Laboratory, Boston, gave the first of three lectures provided by the John Howard Appleton Fund at Brown University, on February 27. The subject of the address was "Alcohol and human efficiency."

DR. HERBERT E. IVES, of the Research Laboratories of the Western Electric Company, read a paper on "The transmission of photographs by telephone," before the American Philosophical Society, Philadelphia, on March 6.

PROFESSOR JAMES F. NORRIS, president of the American Chemical Society and chairman of the division of chemistry and chemical technology, National Research Council, addressed the Chemical Society of Washington, on February 12, on "The reactivity of atoms and groups in organic compounds."

DR. WILLIAM M. CLARK, of the Hygienic Laboratory, Washington, D. C., addressed the scientific staff of the Rockefeller Institute for Medical Research, January 23, on "Oxidation-reduction indicators."

PROFESSOR DAYTON C. MILLER, of the Case School of Applied Science and president of the American Physical Society, delivered three lectures on the subject of "Relativity," at Amherst College, on March 2, 3 and 4.

DR. C. S. HUDSON, of the Bureau of Standards, will present a paper entitled, "Relations between rotatory power and structure in the sugar group," at a joint meeting of a number of the divisions of the American Chemical Society at Baltimore, April 7.

PROFESSOR P. DEBYE, professor of theoretical physics at the University of Zurich, addressed the scientific staff of the Rockefeller Institute for Medical Research, New York, on February 26, on "Hydration and neutral salt action."

DR. RICHARD HAMER, of the department of physics of the University of Pittsburgh, gave an address to the Chiron Club on February 5, on "The application of X-rays to medical science."

PROFESSOR KURT KOFFKA, of the University of Giessen, will give two courses during the first term of the summer quarter of the University of Chicago. His courses will deal with various aspects of the Gestalt psychology. In one course he will emphasize the experimental treatment of this topic, in the second, the genetic treatment.

DR. J. H. JEANS gave the sixteenth Kelvin Lecture to the Institution of Electrical Engineers, England, on February 6, on the subject of "Electrical forces and quanta."

J. W. T. WALSH, of the National Physical Laboratory, Teddington, England, read a paper at the meeting of the Illuminating Engineering Society, London, on January 27, dealing with some little-understood aspects of the effect of shadows in lighting problems.

THE Hunterian oration in connection with the Royal College of Surgeons of England was delivered at the college on February 14, by Sir D'Arcy Power.

A STATUETTE of the late Joseph Swan, executed by his daughter, has been presented to the Institution of Electrical Engineers, of England, by R. K. Morcum.

DR. JOEL HASTINGS METCALF, Unitarian minister of Portland, Maine, and known for his work in photographic astronomy of the minor planets, died on February 21, aged fifty-nine years.

PROFESSOR FREDERICK CRABTREE, head of the department of mining and metallurgical engineering at the Carnegie Institute of Technology, died on February 14, aged fifty-eight years.

DR. CHARLES BINGHAM PENROSE, president of the Philadelphia Zoological Society and formerly professor of gynecology at the University of Pennsylvania, died on February 27, aged sixty-three years.

SIR EDWARD THORPE, the distinguished British chemist, emeritus professor of general chemistry in the Imperial College of Science and Technology, has died in his eightieth year.

DR. HORACE T. BROWN, F.R.S., of England, known for his work on the chemistry of carbohydrates, died on February 6, aged seventy-six years.

DR. E. E. KLEIN, of England, a pioneer worker in bacteriology and histology, died on February 9, at the age of eighty years.

SIR THOMAS CLIFFORD ALLBUTT, the distinguished English clinician, died on February 22, aged eighty-six years.

DR. N. KULCHITSKY, lecturer in histology at University College, London, and formerly professor of anatomy in the University of Kharkov, died on January 29.

JEAN CAMUS, agrégé professor of physiology and physician to the hospitals of Paris, has died, at the age of fifty-two years, as he was about to become the successor of his teacher, C. Richet, as the occupant of the chair of physiology.

AT the annual meeting of the Washington chapter of the American Institute of Chemists, held January 9, the following officers were elected for the ensuing year: *President,* J. F. Couch, Bureau of Animal Industry; *vice-president,* H. E. Patten, consulting chemist, Washington Loan and Trust Building; *secretary,* J. N. Taylor, Bureau of Animal Industry; *treasurer,* H. L. Lourie, U. S. Tariff Commission. Dr. C. E. Munroe was elected honorary president.

THE thirty-fifth annual meeting of the Ohio Academy of Science will be held at the College of Wooster, Ohio, April 3 and 4.

THE twentieth International Congress of Anatomy will be held at Turin from April 6 to 8 under the presidency of Professor G. Romiti. Titles of papers should be sent to Professor G. Levi, Istituto Anatomico, Corso M. d'Azeglio 52, Turin.

PORTLAND, OREGON, will be the place of a regional meeting of the American Society of Mechanical Engineers from June 22 to 25, 1925. The meeting will occupy four full days. There will be two technical sessions in that time at which papers will be presented on the utilization of wood waste, the new 3-inch suction Diesel dredge, the mechanical engineering features of the Long Bell Lumber Company's new saw mill at Longview, Washington, electric logging, steam logging, cable systems in recent logging developments, and the new hydroelectric plant of the Portland Electric Power Company. Excursions have been planned for the last three days of the meeting.

THE Sigma Xi Alumni Association of the University of Pittsburgh held a meeting on February 16, at which the staff of the Mellon Institute presented the following program: Dr. Leonard H. Cretcher, "Derivatives of quinine and acridine of interest to medical science"; E. R. Harding, "New ideas concerning the dietary"; Dr. W. B. Burnett, "Chemistry of accelerators used in vulcanizing rubber"; Dr. Thomas H. Swan, "Vapor pressure measurement of organic solids of low volatility."

THE International Health Board of the Rockefeller Foundation has given the sum of $300,000 to the Danish National Vaccine Institute at Copenhagen, for the purpose of extending the building and laboratories.

THE population of Poland is preparing to raise funds for a national gift to Mme. Curie, the discoverer of radium, according to press dispatches. The gift will take the form of a radium institute in Warsaw, her native city.

THE annual exhibition of the Physical Society of London and the Optical Society was held on January 7 and 8 at the Imperial College of Science and Technology, South Kensington, England.

A GIFT of $20,000 has been made to the University of Pennsylvania by F. M. Kirby, of Wilkes-Barre, Pa., to establish a fellowship in scientific research.

GEORGE EASTMAN, of Rochester, N. Y., has made a gift of $5,000 to the Eugenics Society of the United States of America for promotion of its eugenical undertakings.

THE late Lord Abercrombie, who was himself a distinguished authority on prehistoric archeology, bequeathed to the University of Edinburgh a sum of £17,000 to found a chair in that subject. He also bequeathed to the university certain books to form the nucleus of a special section of the library, together with any other of his archeological and anthropological books and photographs that the library committee of the university might select.

THE San Fernando Nursery Company, of San Fernando, California, has established a department of plant research, for the purpose of studying problems of identification, propagation and improvement of ornamental plants. The work has been placed under the direction of Dr. Arthur D. Houghton, of the University of California.

THE American Institute, of the City of New York, intends to form committees of experts to investigate such problems as government control of radio and the management of the Patent Office as a part of its activities. In connection with these investigations there will be public lectures and open forums for the purpose of informing the public. An exposition of inventions is to be held at the Engineering Societies' Building in New York in the coming spring. This is designed to afford inventors and companies owning or having developed inventions the opportunity of showing the results of their work to the public.

ESTABLISHMENT of the Inventors' International Institute, designed to pass upon the practicability of inventions and the possibility of obtaining patents for them, has been announced. The organization will have an office at 15 Park Row, New York. Members of the institute's board of engineers will include Eustus H. Thompson, L. H. Thullen, Guy D. Collier, New

York; W. S. Richhart, Fort Wayne, Indiana, and Christian E. Brown, Newark, New Jersey.

DR. C. A. SCHENCK, former director of the Biltmore Forest School, will guide the party of students from the Pennsylvania State Forest School at Mont Alto, on their annual tour of the forests of Germany and Switzerland. Dr. J. V. Hofmann, former director of the U. S. Wind River Forest Experiment Station, will have charge of the party, which will consist of not more than twenty men, and will include as many members of other forest schools as can be accommodated. The party will sail from New York on March 28 and Dr. C. A. Schenck will conduct lectures in silviculture and management on the outward voyage, continuing the series of lectures given at Mont Alto early in January. During the tour through the German and Swiss forests with the Mont Alto forest school students last year, Dr. Schenck was able to secure the services of the forester in charge, with the history and working plans, and often invaluable historical maps. The tour will also include some of the French forests.

IN conjunction with the Development Commission, the Departments of Agriculture for England and Wales and Scotland, have instituted a new class of scholarships with the object of training those who desire eventually to take up posts as agricultural organizers under county councils or as lecturers, whether at agricultural departments of universities, agricultural colleges or farm institutes. The scholarships are of two years' duration. The first year of the scholarship will be spent on investigational work in this country and the second year will be spent abroad. The scholarship allowance in the first year will normally be £200; the allowance in the second year will include provision for extra cost of travel and other expenses abroad.

PLANS have been perfected for the establishment of a Bureau of Industrial Standards in the Pennsylvania State Department of Labor and Industry, Harrisburg, to function at an early date. The bureau has been arranged to carry out a policy of having inspectors act as expert advisors in safety work in all branches of manufacture throughout the state; it will be for research, educational and similar purposes, keeping in close touch with the development of standards for health and safety, working, as well, with plant managers and superintendents. Cyril Ainsworth, secretary of the labor department, has been appointed director of the new bureau.

THE *Experiment Station Record* states that the first range livestock experiment station in this country has been established at Miles City, Montana, where a tract of 55,000 acres of grazing land and 2,000 acres of irrigated land formerly occupied by the Fort Keogh Military Reservation was transferred by act of Congress in April, 1924, to the U. S. Department of Agriculture. Buildings and equipment valued at more than $200,000 are available, including two barns built in 1920 at a cost of $12,000 each and about 75 miles of fencing. The station will be devoted to a study of range livestock problems and will be under the immediate supervision of the Animal Husbandry Division of the Bureau of Animal Industry with the cooperation of the Montana Experiment Station and other bureaus and divisions of the department interested in livestock problems. Plans have been made to maintain an initial stock of 1,000 beef feeding cattle, a band of sheep, hogs, horses and turkeys.

UNIVERSITY AND EDUCATIONAL NOTES

THE University of Chicago has received from an anonymous donor the sum of $1,000,000, in connection with its campaign to raise $17,500,000.

CLEVELAND H. DODGE, of New York, president of the Board of Trustees of Robert College, Constantinople, has given $500,000 to the fund for Near East colleges.

THE sum of $100,000 has been given to Boston University by an anonymous donor.

THE governor of New Jersey, in his message to the State Legislature, has recommended appropriations to Rutgers University totalling over $800,000. The items include $200,000 for a new Physics Building for Rutgers College and $200,000 for a new Recitation Building for the College for Women.

DR. LEBARON R. BRIGGS, dean of the faculty of arts and sciences at Harvard University, will resign at the end of the present academic year. His place will be taken by Clifford H. Moore.

DR. WALTER S. HUNTER, head of the department of psychology of the University of Kansas, has been appointed to the G. Stanley Hall chair of genetic psychology at Clark University, established in memory of the first president of the institution, and supported by the income of funds left by him.

DR. HENRY BLUMBERG, of the University of Illinois, has been appointed professor of mathematics at Ohio State University.

DR. ROBERT MAXWELL HARBIN, of Rome, Georgia, has accepted an appointment as professor of orthopedic surgery, Western Reserve University School of Medicine, Cleveland, and orthopedic surgeon to Lakeside and Rainbow Hospitals.

JAMES MCDOWELL, of Boston, a consultant in textile

manufacturing, has been appointed director of research, Textile School, North Carolina State College.

DR. J. A. MILROY has been appointed J. C. White professor of biochemistry, at Queen's University, Belfast, Ireland, and Dr. V. D. Allison lecturer in bacteriology.

DR. PERCY BRIGL, first assistant at the institute of physiological chemistry in the University of Tübingen, has been nominated professor and director of the Institute of Agricultural Chemistry at the Agricultural Hochschule at Hohenheim.

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DISCUSSION AND CORRESPONDENCE

HEMERARCH AND FERALARCH, TWO ADDITIONAL TERMS IN ECOLOGY

IN a paper in the July, 1923, number of *Ecology*, Professor Harshberger[1] proposes a new prefix *hemer* to use in connection with practical or applied ecology.

Although many examples are given of the usefulness of the prefix—a few of which are hemerecology, hemerphysiographic, hemerbiotic, hemerfloristics and hemerrotation—an additional term, *hemerarch*, suggested itself to cover the series of successions taking place on cultivated land or elsewhere where the anthropeic factor is of almost paramount importance.

The contrast thus set up necessitated a corresponding term to designate the genetic series of natural origin. For this purpose the term *feralarch* is proposed—the first part from the Latin *fera*, wild, denoting the absence of the anthropeic factor, the second *arch*, series, as first used by Cooper.[2]

Examples of feralarch series include such xerarch series as that from open sand dunes to tree-covered ground; and such hydrarch series as that from open water to land; while hemerarch series would include such series as a study of the successions among weeds of arable land, crop rotations, etc.

FRANK C. GATES
KANSAS STATE AGRICULTURAL COLLEGE

THE EFFECT OF NOISE ON HEARING

REFERRING to the letter of Dr. G. W. Boot in SCIENCE of October 17, and especially to that of Dr. F. W. Kranz in SCIENCE of December 12, it would seem that the following observations are sufficiently germane to be of interest. It chances that your correspondent has suffered from defective hearing in both ears since boyhood. The secondary complication which seems to be immediately responsible for the impairment of hearing is the nearly complete destruction of the tympanic membrane of one ear, and its deformation in the other, with of course more or less accompanying ankylosis of the ossicles in each instance. The failure of function becomes therefore a purely mechanical matter due wholly to injury to the delicate machinery which transmits the perceptible vibrations to the receptor itself. The true auditory organ remains unimpaired.

Now my personal experience ever since I can remember to have thought about it has been that in the presence of *heavy* vibrations—on board a moving trolley car, in a power house, in an automobile, and especially on board a moving train—my hearing at once becomes sensibly more acute, often quite surprisingly so. On frequent occasions, when traveling in a Pullman car, I have heard whisperings, never intended for my ears, on the part of people in neighboring seats, which at ordinary times would have been well below my perceptive horizon, but now embarrassed me by the distinctness of the words. In such situations ordinary conversation is understood by me with the utmost ease, and I frequently find myself forced to request my seatmate to reduce his voice, the while he is just as apt to be asking me to elevate mine. During intervals when the train comes to rest, the conversation becomes immediately unintelligible to me and is best held in abeyance until the train sees fit to start up again.

More than once on board train I have participated in the identical experiment suggested by Dr. Kranz, *i.e.*, conversation with another person subject to an impediment of hearing similar to or greater in degree than my own. If the organ of Corti and the auditory nerve are unaffected the consequences have generally been the same as I have related except that the phenomena described become mutual, little adjustment to one another's peculiarities becomes necessary, and normal people usually find our conversation carried on far too quietly for them. A somewhat intuitive lip reading is a considerable aid to me in everyday life, but in such situations as these I do not have recourse to it or need it.

A similar phenomenon becomes apparent at symphony orchestra concerts. Instruments of relatively softer tone or lower pitch, which I attend with difficulty in the quieter parts of the program, I am often able to pick out with exquisite clearness against the rich tonal background of the full orchestra in fortissimo. As a consequence I always enjoy numbers for the full orchestra best. Also at such times the

[1] Harshberger, John W., "Hemerecology: the ecology of cultivated fields, parks and gardens," *Ecology*, 4: 297–306, 1923.

[2] Cooper, Wm. S., "The climax forest of Isle Royale, Lake Superior, and its development," *Bot. Gaz.*, 55: 1–44, 115–140, 189–235, 1913.

whisperings and program-rustlings of those about me become sharply evident and very distracting.

What the true explanation for all this may be I can not presume to state, but it has seemed reasonable to me to believe that heavy vibrations occurring in force and in more or less regular succession or practical continuity tend to jar the stiffened transmission mechanism into a vibration or state of sensitivity of its own, when it is able to pick up and conduct those lesser vibrations of a higher pitch which alone would be quite incapable of activating it. Conversely, where the auditory receptor mechanism itself is involved in injury, such phenomena could hardly be expected to occur.

Doubtless pretty much all of what I record is commonplace enough in the appropriate literature and certainly in the experience of those similarly afflicted, but the flat statement by one of your correspondents that reports of this type are fallacious and the uncertainty evident in some of the replies thereto induce me to offer these few notes of a directly empirical nature with an apology for the unavoidable personal element.

B.

ON STUDENT MISINFORMATION

IN connection with the examples of student misinformation in SCIENCE for December 19, 1924, some experiences of my own may be of interest. The perpetrators in each case were college graduates.

In discussing an old case in which the report said, "Plea *son assault demesne,*" I asked a student, "What does that plea mean?" He looked at it and replied, "I did not look up that Latin phrase." When I asked, "Are you sure it is Latin," he answered, after another look, "I did not look up that Anglo-Saxon phrase."

A type of case often referred to considers how far one who stands in no relation to another may be bound legally in an emergency to act as a good Samaritan. After these cases had been discussed repeatedly, a student came to me with his note book and explained that in reviewing he found many references to the "good Sarmatian cases"—would I be good enough to tell him what these "good Sarmatian cases" were?

Another much discussed case involved an agreement to build a silo. As to this a student wrote: "I do not know what a silo is—but I will assume it is some kind of barn."

So long as "making acquaintances," and "contacts," and extra-curriculum "activities" are the realities of student life, and the work of lecture room and laboratory is a mere ritual—in the words of Terence Mulvaney "an impartinint and shuparfluous necisity"—we must continue to expect these things.

ROSCOE POUND

HARVARD UNIVERSITY

IN regard to the discussion in your recent issues on student "howlers," is it not well to remember that a course in science or any other subject is a game between the students and the instructor? It is an unfair game also, for the instructor makes the rules, plays on one side and then acts as referee, umpire and score-keeper. Naturally, students feel somewhat at a disadvantage, and we can hardly expect them to take a vital interest, nor should we expect that they will refrain from devious practices to beat a game rigged against them. These comments apply equally whether the lecture or laboratory system is used. To the student the excessive authority and arbitrary power of the teacher seems a bar to ordinary intercourse or common interest. If the teacher is interested in a subject, that subject is *ipso facto* abhorrent to the student. Successful teaching is a matter of personality by which the teacher overcomes with the force of his enthusiasm and mental energy the natural disadvantages of his position. Successful teachers tend to overemphasize the particular devices, stratagems and systems by which they have at various times stimulated real thought in reluctant minds. The unsuccessful also rely on some system or systems as if they were fetiches by which the spirit of scholarship might be invoked. Yet, if we will honestly review our recollections of our own teachers, we will realize that those who taught us most were those whose personalities were to us the most impressive. The great teachers need no system; the others should be eclectic, for they can make up for deficiencies in personality by the use of many devices and by an occasional change of pace.

KIRK BRYAN

U. S. GEOLOGICAL SURVEY,
WASHINGTON, D. C.

ANTHROPOLOGICAL STUDIES ON THE NATIVES OF THE YENISEI RIVER

PROFESSOR VASSILIJ IVANOVIČ ANUČIN, of the University of Kazan, Russia, writes as follows:

During my 1905–1909 expedition to the Yenisei Ostiaks I gathered immense scientific material, which so far I have not been able to publish or even fully prepare for publication, due to our financial conditions. The material is partly linguistic, partly ethnological. The people studied are disappearing. In 1907 they still numbered 900 individuals; in 1923 there remained less than 100. Moreover, I have recently learned that this remnant has now practically lost its special ethnic character. I am the only one who has thoroughly studied them and espe-

manufacturing, has been appointed director of research, Textile School, North Carolina State College.

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THE EFFECT OF NOISE ON HEARING

REFERRING to the letter of Dr. G. W. Boot in SCIENCE of October 17, and especially to that of Dr. F. W. Kranz in SCIENCE of December 12, it would seem that the following observations are sufficiently germane to be of interest. It chances that your correspondent has suffered from defective hearing in both ears since boyhood. The secondary complication which seems to be immediately responsible for the impairment of hearing is the nearly complete destruction of the tympanic membrane of one ear, and its deformation in the other, with of course more or less accompanying ankylosis of the ossicles in each instance. The failure of function becomes therefore a purely mechanical matter due wholly to injury to the delicate machinery which transmits the perceptible vibrations to the receptor itself. The true auditory organ remains unimpaired.

Now my personal experience ever since I can remember to have thought about it has been that in the presence of *heavy* vibrations—on board a moving trolley car, in a power house, in an automobile, and especially on board a moving train—my hearing at once becomes sensibly more acute, often quite surprisingly so. On frequent occasions, when traveling in a Pullman car, I have heard whisperings, never intended for my ears, on the part of people in neighboring seats, which at ordinary times would have been well below my perceptive horizon, but now embarrassed me by the distinctness of the words. In such situations ordinary conversation is understood by me with the utmost ease, and I frequently find myself forced to request my seatmate to reduce his voice, the while he is just as apt to be asking me to elevate mine. During intervals when the train comes to rest, the conversation becomes immediately unintelligible to me and is best held in abeyance until the train sees fit to start up again.

More than once on board train I have participated in the identical experiment suggested by Dr. Kranz, *i.e.*, conversation with another person subject to an impediment of hearing similar to or greater in degree than my own. If the organ of Corti and the auditory nerve are unaffected the consequences have generally been the same as I have related except that the phenomena described become mutual, little adjustment to one another's peculiarities becomes necessary, and normal people usually find our conversation carried on far too quietly for them. A somewhat intuitive lip reading is a considerable aid to me in everyday life, but in such situations as these I do not have recourse to it or need it.

A similar phenomenon becomes apparent at symphony orchestra concerts. Instruments of relatively softer tone or lower pitch, which I attend with difficulty in the quieter parts of the program, I am often able to pick out with exquisite clearness against the rich tonal background of the full orchestra in fortissimo. As a consequence I always enjoy numbers for the full orchestra best. Also at such times the

[1] Harshberger, John W., "Hemerecology: the ecology of cultivated fields, parks and gardens," *Ecology*, 4: 297–306, 1923.

[2] Cooper, Wm. S., "The climax forest of Isle Royale, Lake Superior, and its development," *Bot. Gaz.*, 55: 1–44, 115–140, 189–235, 1913.

whisperings and program-rustlings of those about me become sharply evident and very distracting.

What the true explanation for all this may be I can not presume to state, but it has seemed reasonable to me to believe that heavy vibrations occurring in force and in more or less regular succession or practical continuity tend to jar the stiffened transmission mechanism into a vibration or state of sensitivity of its own, when it is able to pick up and conduct those lesser vibrations of a higher pitch which alone would be quite incapable of activating it. Conversely, where the auditory receptor mechanism itself is involved in injury, such phenomena could hardly be expected to occur.

Doubtless pretty much all of what I record is commonplace enough in the appropriate literature and certainly in the experience of those similarly afflicted, but the flat statement by one of your correspondents that reports of this type are fallacious and the uncertainty evident in some of the replies thereto induce me to offer these few notes of a directly empirical nature with an apology for the unavoidable personal element.

B.

ON STUDENT MISINFORMATION

IN connection with the examples of student misinformation in SCIENCE for December 19, 1924, some experiences of my own may be of interest. The perpetrators in each case were college graduates.

In discussing an old case in which the report said, "Plea *son assault demesne*," I asked a student, "What does that plea mean?" He looked at it and replied, "I did not look up that Latin phrase." When I asked, "Are you sure it is Latin," he answered, after another look, "I did not look up that Anglo-Saxon phrase."

A type of case often referred to considers how far one who stands in no relation to another may be bound legally in an emergency to act as a good Samaritan. After these cases had been discussed repeatedly, a student came to me with his note book and explained that in reviewing he found many references to the "good Sarmatian cases"—would I be good enough to tell him what these "good Sarmatian cases" were?

Another much discussed case involved an agreement to build a silo. As to this a student wrote: "I do not know what a silo is—but I will assume it is some kind of barn."

So long as "making acquaintances," and "contacts," and extra-curriculum "activities" are the realities of student life, and the work of lecture room and laboratory is a mere ritual—in the words of Terence Mulvaney "an impartinint and shuparfluous necisity"—we must continue to expect these things.

ROSCOE POUND

HARVARD UNIVERSITY

IN regard to the discussion in your recent issues on student "howlers," is it not well to remember that a course in science or any other subject is a game between the students and the instructor? It is an unfair game also, for the instructor makes the rules, plays on one side and then acts as referee, umpire and score-keeper. Naturally, students feel somewhat at a disadvantage, and we can hardly expect them to take a vital interest, nor should we expect that they will refrain from devious practices to beat a game rigged against them. These comments apply equally whether the lecture or laboratory system is used. To the student the excessive authority and arbitrary power of the teacher seems a bar to ordinary intercourse or common interest. If the teacher is interested in a subject, that subject is *ipso facto* abhorrent to the student. Successful teaching is a matter of personality by which the teacher overcomes with the force of his enthusiasm and mental energy the natural disadvantages of his position. Successful teachers tend to overemphasize the particular devices, stratagems and systems by which they have at various times stimulated real thought in reluctant minds. The unsuccessful also rely on some system or systems as if they were fetiches by which the spirit of scholarship might be invoked. Yet, if we will honestly review our recollections of our own teachers, we will realize that those who taught us most were those whose personalities were to us the most impressive. The great teachers need no system; the others should be eclectic, for they can make up for deficiencies in personality by the use of many devices and by an occasional change of pace.

KIRK BRYAN

U. S. GEOLOGICAL SURVEY,
WASHINGTON, D. C.

ANTHROPOLOGICAL STUDIES ON THE NATIVES OF THE YENISEI RIVER

PROFESSOR VASSILIJ IVANOVIČ ANUČIN, of the University of Kazan, Russia, writes as follows:

During my 1905–1909 expedition to the Yenisei Ostiaks I gathered immense scientific material, which so far I have not been able to publish or even fully prepare for publication, due to our financial conditions. The material is partly linguistic, partly ethnological. The people studied are disappearing. In 1907 they still numbered 900 individuals; in 1923 there remained less than 100. Moreover, I have recently learned that this remnant has now practically lost its special ethnic character. I am the only one who has thoroughly studied them and espe-

cially their language. I may not live very long, nor do I wish to do so; but I fear that unless these data can be properly elaborated and published there would be lost with me to science a good deal that could not be replaced. My monthly salary is 48 rubles. In order properly to work up my data I would need about 250 rubles (approximately $125) a month for one year. The result would be a book of about 500 pages on the Yenisei Ostiaks. For the illustrations I have more than 100 photographs and 10 drawings, besides 40 *aquarelles* made under my direction by a Russian artist. These *aquarelles* alone would be a valuable acquisition for any museum.

It is to be hoped that Professor Anučin will find the help of which he is in need.

A. HRDLIČKA

U. S. NATIONAL MUSEUM

SCIENTIFIC BOOKS

A Handbook of Solar Eclipses. By ISABEL M. LEWIS. XI + 118 pp. Duffield & Co., New York. Price, $1.25.

THIS little book was undoubtedly one of the best sellers in New England and New York just before and after the eclipse of January 24.

It is intended to enable the layman to make the most of the few precious seconds of a total eclipse of the sun. It explains in non-technical language the cause of eclipses and describes clearly what to look for during an eclipse. There are chapters on the shadow bands, Baily's beads, the chromosphere, prominences and corona and general instructions for viewing a total eclipse. Herein are answers to most of the questions with which astronomers are bombarded before every eclipse.

There are also chapters of a somewhat more technical nature on the prediction of eclipses, the flash spectrum, the astronomer's eclipse program and the scientific importance of eclipses. A bit of history dealing with the noted eclipses of the past and a chapter on the total solar eclipses of the near future conclude the book. Special attention is given to the eclipses of January 24, 1925, January 14, 1926, and June 29, 1927. The path of the 1927 eclipse crosses northern England. It will be the first total eclipse of the sun to occur in the British Isles since 1724.

The book is well illustrated by reproductions of photographs of the eclipses of 1918, 1922 and 1923.

Mrs. Lewis has rendered a real service by putting this rather difficult subject into clear and simple language. A second edition with illustrations of the 1925 eclipse will undoubtedly be as popular in England in 1927 as the first edition has been here this year.

FREDERICK SLOCUM

WESLEYAN UNIVERSITY

LABORATORY APPARATUS AND METHODS

A METHOD OF DEMONSTRATING MESONEPHRIC TUBULES

IT is often of considerable advantage to the teacher of embryology or histology to be able to show the contour and extent of the structures being studied in sections. The method described here gives an excellent outline of mesonephric tubules and the preparation may be made in a very short time. A somewhat similar technique was employed in the study of the elimination of iron by the mesonephros of Necturus.[1] The essential feature of the method is the precipitation of the iron as Prussian blue in the lumina of the kidney tubules.

Chase[2] has shown that in the pelvic (secretory) portion of the mesonephros, there are two sets of tubules (primary and secondary) which have direct connections with the body cavity by way of outer segments (peritoneal canals) and nephrostomes. Substances placed in the body cavity accordingly find their way into the primary and secondary tubules by this route and are eventually eliminated, in part at least, through the Wolffian duct.

A balanced mixture of sodium ferrocyanide and ammonium ferric citrate, made by adding 10 parts by volume of a 3 per cent. solution of the former to 7 parts by volume of a 4 per cent. solution of the latter[3] was kept as a stock solution. A quantity of this was diluted 10 times and a sufficient amount injected into the body cavity to produce a mild distention. The animals were left from two to six hours and then killed by immersion in an aqueous solution of chloretone. Sufficient time should elapse before killing to allow the iron salts to at least reach the Wolffian duct. The time needed for this is variable. The mesonephroi are dissected free and fixed in a solution sufficiently acid to produce the Prussian blue reaction. Acid-formalin or Gilson's fluid is satisfactory. The kidneys may be kept in alcohol and studied as opaque objects or may be cleared. If cleared, benzol or toluol is preferred, as they remove some of the pigment present. Later the tissue may be transferred to some less volatile fluid as oil of wintergreen.

On the ventral surface of the preparation, the neck, distal to its junction with the peritoneal canal, the proximal convoluted portion, the narrow straight part and the distal convoluted portion of a tubule can be readily followed by means of the dense blue deposit in the lumen. On the dorsal surface, the short junctional portions, collecting tubules and the Wolffian

[1] Dawson, A. B., 1925, *Am. Jour. Physiol.*, in press.

[2] Chase, S. W., 1923, *Jour. Morphol.*, Vol. 37, p. 457.

[3] Collip, J. B., 1920, Univ. Toronto Studies, Physiol. Series, No. 35.

duct can be easily seen. The primary and secondary tubules are so isolated that little overlapping occurs and a clear conception of the form and extent of an individual tubule is obtained. In this respect the preparation made by intraperitoneal injection is much more instructive than one made by injections through the Wolffian duct in which every tubule contains more or less color mass.

ALDEN B. DAWSON

LOYOLA UNIVERSITY
SCHOOL OF MEDICINE

SPECIAL ARTICLES

WAVE-LENGTH SHIFTS IN THE SCATTERING OF LIGHT[1]

BOHR[2] has called attention to a remarkable prediction made by Smekal[3] and by Kramers and Heisenberg (unpublished), to the effect that when monochromatic light falls on a multiply periodic electromagnetic system, the scattered radiation may contain not only the incident frequency, but also combinations of this frequency with those characteristic of the scatterer. The purpose of this note is to direct attention to a conclusion pointed out by one of the writers some two years ago that the wave-length shift may be very large in certain favorably chosen cases of the scattering of ordinary light. It appears that a part of this frequency shift is the quantum analogue of the phenomenon predicted by Kramers and Heisenberg. In general, the wave-length change is due to three influences—the Doppler shift caused by the initial motion of the atom, the Compton shift due to the recoil of the atom or the ejected electron, and the shift due to alteration in the internal energy of the atom. To a first approximation these are additive.

A. H. Compton's original theory[4] of the wave-length shift of scattered radiation was based on the assumption that each unidirectional quantum of incident radiation is deflected by a single electron, which recoils from the momentum of the quantum. For scattering at angle φ with the primary beam the increase in wave length (in centimeters) should be

$$\Delta\lambda = \frac{2h}{mc}\sin^2\frac{\varphi}{2} \qquad (1)$$

where h, m and c are Planck's constant, the mass of the electron, and the velocity of light, respectively. In Ångstrom units the shift is

$$\Delta\lambda = .0484\sin^2\frac{\varphi}{2}$$

Ross[5] attempted, without success, to observe this shift in the visible spectrum by scattering the light of the mercury line 5461 A from paraffin, and by multiple reflections from silvered glass.

We shall not attempt to discuss the latter experiment, but there are at least two possible reasons why no shift of the order of .02 A was observed in the case of paraffin. First, it seems improbable that there are "free" electrons in paraffin, and presumably light of wave length 5461 A is unable to eject electrons from atoms of this substance. Under such conditions the scattering must be attributed to the atom as a whole (or even to larger aggregates) and the shift is inappreciably small. Second, even if electrons were ejected, the shift should not be given by the equation (1); for in its derivation both the work required to separate the scattering electron from its parent atom, and the final momentum of the atom, were neglected. Compton[6] has taken these influences into account in a revised theory. There are not enough equations to determine all the unknowns in this problem. For example, the recoiling atom and electron as well as the scattered quantum will in general possess angular momentum with respect to the center of gravity of the system. The distribution of this angular momentum can not be specified unless we know the dynamics of the collision. The result is that the momentum of the atom and the direction cosines of its trajectory, which are unknown, appear in the formula for $\Delta\lambda$. In all probability these quantities are dependent on the relative phases of the incident quantum and the internal motions of the atom, so that $\Delta\lambda$ is not a constant for a given frequency, angle of scattering and material. This circumstance is probably contributory to the fact that the shifted peak is wider than the unshifted one. At any rate, the formula shows that $\Delta\lambda$ lies between infinity and $\lambda^2/(\lambda-\lambda_s)$, where hc/λ_s is the ionization potential for the atomic energy level from which the scattering electron is ejected. That is, $\Delta\nu$ lies between $-\nu$ and $-\nu_s$. In unpublished calculations of Ruark and Ellett these results are still further extended, though the numerical result is not changed appreciably. A formula is derived for the frequency ν_2 obtained when a needle quantum of frequency ν_0 falls on an atom and is scattered without ejecting an electron. Let E_0 be the rest-energy of the atom and let it have a velocity $v_0=\beta_0 c$, making an angle ϑ_0 with the direction of the quantum. E_2, β_2 and ϑ_2 denote the corresponding quantities for the final state of the

[1] Published by permission of the Director of the Bureau of Standards, U. S. Department of Commerce.

[2] *Naturwissenschaften* 12, 1115, 1924.

[3] *Naturwissenschaften* 11, 873, 1923.

[4] *Bull.* Nat. Research Council, Vol. 4, No. 20, 1922.

[5] *Proc. Nat. Acad. Sci.*, 9, 246, 1923.

[6] Compton, *Phys. Rev.*, 24, 168, 1924.

atom, while E_1 is the rest-energy which the atom would possess if it absorbed and retained the incident quantum. Then

$$\nu_0 \frac{1-\beta_0 \cos \vartheta_0}{\sqrt{1-\beta_0^2}} \frac{2hE_0}{E_1^2-E_0^2} = \nu_2 \frac{1-\beta_2 \cos \vartheta_2}{\sqrt{1-\beta_2^2}} \frac{2hE_2}{E_1^2-E_2^2}$$

When the values of β_2, E_1, etc., obtained from the solution of the dynamical problem are put in this equation, it specifies the entire frequency change caused by the initial motion of the atom, its recoil from the quantum and the change in its quantized energy. Just as in Compton's revised theory, the shift will be indeterminate if we suppose that an electron is ejected.

It is quite possible that this wave-length change is the cause of some of the mysterious continuous spectra lying in the region of the ordinary spectrograph. Suppose, for example, that a vapor for which $\nu_0 = 40{,}000$ cm.$^{-1}$ is illuminated with light of wave length 2,000 A. That part of the scattered radiation which is not of wave length 2,000 A will lie at frequencies lower than 10,000 cm.$^{-1}$, corresponding to 1 μ. In other words, it will appear as radiant heat. Shorter incident wave lengths might give a spectrum extending into the visible. Therefore, it seems important to photograph the spectra of vapors which are scattering intense radiation of short wave lengths.

It may be pointed out that spark lines having frequencies greater than the highest series limit of the neutral atom should give rise to a scattered line spectrum provided that $\Delta\lambda$ does not depend on the relative phases of the atom and the incident quantum. Spectrum lines of this character would be strictly analogous to the displaced lines of Kramers and Heisenberg. Apparently such lines do not exist in the spectra of the alkaline earths.

It remains for us to consider whether the scattered combination frequencies of Kramers and Heisenberg can be detected experimentally. When sodium vapor is illuminated with light of the second member of the sodium principal series, the resonance radiation contains both the first and second members of the series (and presumably 4 pairs of lines in the infrared). Bohr states that phenomena of this kind constitute a special case of those predicted by Kramers and Heisenberg. Here the D lines are to be considered as a difference frequency, in accordance with the equation

$$1s - 2p = (1s - 3p) - (2p - 3p).$$

The emission of a summation frequency may be obtained by illuminating mercury atoms in the $2p_1$ state with light of the line $2p_1 - 3d_2$, for example. The emitted light will contain $2p_2 - 3d_2$, and

$$2p_2 - 3d_2 = (2p_1 - 3d_2) + (2p_2 - 2p_1).$$

In these instances the atomic resonator is so sharply tuned that a very slight deviation from the correct frequency causes the amount of scattering to decrease tremendously. Similar remarks apply to molecular resonance spectra excited by monochromatic light. It seems very doubtful that the combination frequencies can be detected in the case of ordinary Rayleigh scattering.

PAUL D. FOOTE
ARTHUR EDWARD RUARK

BUREAU OF STANDARDS
WASHINGTON, D. C.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

BOTANICAL SCIENCES AT THE WASHINGTON MEETING

(A report for Section G appeared in Science for February 6.)

The Botanical Society of America

President, William Crocker.

Secretary, Ivey F. Lewis, University of Virginia, University, Va.

(Report by Ivey F. Lewis, unless otherwise noted)

The Washington meeting of the Botanical Society of America was the most largely attended and one of the most successful in the history of the society. The names of 88 new members were added to the roll of the society, and the following corresponding members were elected: Professors V. H. Blackman and A. C. Seward, of England, and Karl Goebel, of Germany. The officers elected for 1925 are: J. R. Schramm, *president*; W. J. V. Osterhout, *vice-president*; G. E. Nichols, *treasurer*. C. E. Allen was elected to represent the society in the National Research Council, Division of Biology and Agriculture, and H. C. Cowles was elected to the editorial board of the *American Journal of Botany*. The society endorsed the proposed deep sea exploration of the United States Navy. Forward steps were also taken in conjunction with the Ecological Society of America and the American Phytopathological Society, looking toward the convening of an International Botanical Congress in Ithaca in August, 1926. The exhibits by members proved exceptionally interesting and attractive. These included, among others, cytological preparations by W. R. Taylor, R. E. Cleland and A. M. Showalter, and a very fine exhibit of plant pigments by F. M. Schertz. The Botanical Society will meet with the American Association in Kansas City in 1925. The program, as usual, was carried on by sectional groups.

General Section, B. S. A.

Chairman, D. S. Johnson; *secretary,* W. R. Taylor.

The papers before the General Section covered, as usual, a wide range. The algae were particularly well represented with seven papers, covering the cytology of Sargassum and Nitella, a new type of red snow algae, a new Oedocladium, distributional studies in Massachusetts and Florida, photoperiodism in Chara and periodicity in Dictytota. Morphological studies included papers on Batis, Quercus, Petunia, Podophyllum, Poa, Xanthium, the peanut, roots and rootstocks, abscission in woody plants, the development of vessels from cambium, cleavage polyembryony in Conifers, plant galls, and Devonian fossils mostly of Cordaitean affinities. Cytological studies were reported on Zea, Tradescantia, Gasteria and other genera, while a particularly full account of fertilization in Riccardia and Fossombronia was given by A. M. Showalter. Other papers covered the local flora of Cold Spring Harbor, fifty years changes in the flora of Penikese Island, the biology of *Lewisia rediviva,* and the position of botany in the college curriculum, with a completion test in biology for use in college classes. The following officers were elected: *Chairman,* R. B. Wylie; *vice-chairman,* J. T. Buchholz; *secretary-treasurer,* P. B. Sears. At the joint session with the Ecological Society of America, twelve papers were given. Descriptions were given of ecological conditions in the Mediterranean region, Brazil, Texas, the Amazon Valley and the western Himalayas. Other subjects were the principles of competition as illustrated by the growth of wheat, the grouping of plants, the effect of salinity in determining the distribution of plants on river estuaries, animal communities of an Illinois forest, enlarged trunk bases in black ash and the ecology of Scolopendrium. P. L. Ricker spoke on the preservation of our native flora and reported on the proposed national arboretum and botanical garden in Washington.

Mycological Section, B. S. A.

Chairman, H. S. Jackson

The program of the Mycological Section included taxonomic studies on Astrocystis, Conidiobolus, Uredinales, Helicodesmus and Choanephora. Papers of a physiological nature were read on Phytophthora and Penicillium. Life-history studies were presented on Pilobolus, Panaeolus, certain malodorous fungi, the stromatic Sphaeriales and Dictyuchus. The program also included papers on the cytology of Puccinia and heteroecism in rusts. The joint session of the Mycological Section with the American Phytopathological Society included studies of the citrus scab organism, Phytophthora, leaf spots of maize, bur clover and related plants, bud rot of the coconut, mosaic, crowngall and the genera Phoma and Macrophoma. An outstanding paper of the section was the report by A. H. Reginald Buller of the work of his students on sex in mushrooms and toadstools, the beauty of the experiments and the clearness and definiteness of the results being perhaps unique in this difficult field. E. M. Gilbert was elected chairman of the section for 1925.

Systematic Section, B. S. A.

Chairman, William R. Maxon; *secretary,* Agnes Chase

(*Report by W. R. Maxon and I. F. Lewis*)

The formal program, of three sessions, included a discussion of experimental methods in taxonomy, the progress of classification of cultivated plants and the taxonomic characters of Helianthus. H. M. Hall's paper on "Experimental methods in taxonomy" attracted much attention. W. W. Eggleston gave a biographical account of William F. Cusick, the Oregon botanist. A special session was devoted to a round-table discussion of the training of systematists in college, university and research institutions. Fruitful points of view were presented, and it was voted to publish a summary of the impromptu discussion. A committee was appointed to study and report on this subject at the next annual meeting, consisting of K. M. Wiegand, M. A. Howe and C. R. Ball. Of the greatest interest to the section was the report of the chairman of the Standing Committee on Nomenclature, A. S. Hitchcock. After full discussion the following resolution was recommended to and at a later business meeting unanimously passed by the Botanical Society:

> The Botanical Society of America recommends favorably the appointment, by the next International Botanical Congress, of an International Interim Committee of specialists in nomenclature to consider the Resolutions on Nomenclature adopted by the Imperial Botanical Conference held in London in 1924, the typification of the present list of *nomina conservanda,* and other matters of nomenclature, for report to the succeeding congress.

Physiological Section, B. S. A.

Chairman, W. J. Robbins.

Secretary-treasurer, S. C. Brooks, Hygienic Laboratory, Washington, D. C.

(*Report by S. C. Brooks*)

A meeting of all those interested in plant physiology was held on Monday morning, December 29, over one hundred being present. At this meeting Dr. Crocker, representing the majority of the committee appointed to investigate the proposed dissolution of the section, reported that sentiment was very strongly in favor of its continuance. This report was adopted by the section and later by the society, and will be

more fully presented in a report of the latter. Sessions of the Physiological Section for the reading of papers were held Monday afternoon, Tuesday morning and Wednesday afternoon, with an average attendance of about fifty. Thirty papers were read.

The optimum pH for the growth of *Lemna minor* was shown by F. B. Wann to lie between 5.0 and 6.0. *Chlorella* sp. was also grown in nutrient solutions by Dr. Wann and E. F. Hopkins, and shown to grow best at about the same reaction as *Lemna,* namely, pH 5.73. Sophie Satina and A. F. Blakeslee pointed out a very high but not absolute correlation between + sexuality and reducing power among Mucors; sodium tellurite was used as an oxidation-reduction indicator. The more nitrogen a seed contains the greater is shoot growth as compared to root growth, and this, as Mary E. Reid explained, is largely independent of species or family.

At the meeting Tuesday morning G. W. Scarth discussed the swelling of gelatin: the resistance to stretch is affected by varying pH in much the same way as the swelling; gels of equal concentration obtained by allowing dry gelatin to swell in water and by allowing hot sols to cool were of unequal elasticity. E. F. Hopkins showed that at low temperatures respiration and total sugar content of potato tubers ran parallel. Wounding produced similar parallel changes in sugar content and respiration; there is apparently a close correlation between sugar concentration and respiration. H. W. Popp, using an admirably thorough technique, gave conclusive evidence that, total radiant-energy supply being constant, sunlight ultra-violet could be eliminated without any noticeable effect on the growth or habit of a wide variety of vascular plants; but that elimination of all light of wave lengths shorter than the blue-green produced marked changes in their growth and composition. Similarly, thorough experiments by John M. Arthur, H. W. Popp and James E. Webster showed the influence of the intensity and duration of light under controlled environmental conditions, with and without enrichment of the air with CO_2.

An extra session was held Wednesday afternoon to complete the scientific program. A group of papers by T. Morinaga and by W. E. Davis treated of the germination of seeds. Dr. Morinaga showed that alternating periods of warmth and coolness accelerated germination of many seeds. This effect was often masked if the seeds were germinated under water, which also promoted germination of many species; reduction of the oxygen tension also accelerated germination of some seeds, masking the effects of immersion and alternating temperatures, and was apparently a fundamental factor. Certain seeds required increased oxygen tension when immersed. Studies on catalase content and on the effect of removing the outer coats of certain seeds led W. E. Davis to conclude that germination is dependent upon optimum ventilation of the seed. A paper upon the conditions affecting germination of the spores of *Onoclea sensibilis* presented by Constance E. Hartt aroused much interest, as did also the studies of D. M. Moore upon breaking the dormancy of tulips by the use of low temperatures, and the ingenious experiments of W. J. Himmel on the mechanical characteristics of *Podophyllum* petioles.

After the conclusion of the scientific program plans for the further development of the section were discussed and referred to the Plant Physiological Board for final action.

Genetics Section, B. S. A.

This section operates with the Genetics Section of the American Society of Zoologists, the two forming the Genetics Sections, reported below.

The American Phytopathological Society

President, F. D. Fromme.

Secretary, R. J. Haskell, U. S. Department of Agriculture, Washington, D. C.

(*Report by E. C. Stakman and F. J. Schneiderhan*)

If attendance is a reliable criterion the interest and zealousness of the members of the society is showing a healthy yearly growth. All records for attendance were broken at the sixteenth annual meeting. A total attendance of approximately 226 was registered. This is 34 per cent. of the membership, which now numbers 668. The attendance percentages of meetings of the society in recent years at Cincinnati, Boston and Toronto are 140 (25 per cent.) 95 (17 per cent.) and 85 (16 per cent.), respectively. Forty-nine papers were presented at the regular meetings. Compared to the 114 papers presented at the Cincinnati meeting it is evident that the number presented at the Washington meeting was less than half. This, however, is a fortunate result of the action taken at the Cincinnati meeting to limit the number of papers to completed or nearly completed research and not to include progress reports. Furthermore, the ruling requiring abstracts to be sent in a month earlier than hitherto also reduced the number of papers. The average quality of the papers was this year very high. The average time allotted to each speaker was longer and the general arrangement of the program, particularly the division of the society on January 1 into three sections for vegetable, cereal and fruit diseases, was productive of good results in crystallizing interest and expediting the presentation of the program.

Two joint sessions were held, one with Section G of the American Association for the Advancement of

Science and one with the Mycological Section of the Botanical Society of America. Of interest to phytopathologists were two symposia held by the Potato Association of America, in which the problem of potato improvement and the results of dusting and spraying experiments on potatoes were discussed. A conference of extension workers in plant pathology was held on Tuesday afternoon, led by F. C. Meier. The 49 papers presented at the regular sessions of the society may be conveniently classified as follows: vegetable diseases, 16 papers; cereal, 11; fruit, 8; mosaic, 4; fiber crops, 2; forage, 1; taxonomic, 2; technique, 2, and miscellaneous papers, 3. A brief digest of some of the more noteworthy papers follows: G. N. Hoffer and J. T. Trost pointed out a striking relation between severity of infection by root rot of corn plants grown under conditions of balanced nutrient supply and with deficiency of phosphates and potash. Infection was markedly greater when phosphates and potash were deficient. W. H. Tisdale and V. F. Tapke demonstrated for the first time that infection by loose smut of barley resulted from inoculating seed with spores. Floral infection had hitherto been considered to be the only known method of infection. F. R. Jones divides the soil-inhabiting fungi parasitic on peas into three groups based on the part of the plant invaded. The important point he made was that *Aphanomyces euteiches* Drechsler was the most widespread cause of destructive root rot of peas. Resistance to three different diseases in rye appears to be due to separate factors independently inherited. Resistance to each disease is dominant in rye according to E. B. Mains. A. G. Newhall showed that tip-burn of lettuce is not of bacterial origin but is due to disturbed physiology resulting from fluctuating temperature and moisture in the presence of readily available potassium and nitrogen. Donlad Folsom and Reiner Bonde gave evidence that *Alternaria solani* can cause tuber rot of potato. A new and ingenious method of determining the approximate number of ascospores of the apple scab fungus in the air was described by G. W. Keitt. In two papers F. J. Schneiderhan pointed out the correlation between ascospore discharge and seasonal infection by apple scab, the direct relation of rainfall to ascospore discharge and the occurrence of three definite cycles of infection in the early growing season. The first detailed knowledge regarding the effect of removing bitter-rot mummies and infected fruit in apple orchards was presented by R. H. Hurt. He showed that seasonal infection can be reduced to a minimum by this removal. S. M. Dietz amplified our knowledge of the relation of crown rust of oats to its alternate hosts of the four Rhamnaceous genera, Berchemia, Ceanothus, Zizyphus and Rhamnus. He also showed that resistance to oat stem rust is dominant in crosses of eight different pure lines of oats. Flax rust and wilt resistance are not correlated. Rust resistance is dominant and probably can be combined with satisfactory morphological characters in flax, according to a paper by A. W. Henry and E. C. Stakman. The minimum, optimum and maximum temperatures of flax rust spores together with ecologic factors were worked out by Helen Hart. Perley Spaulding presented results of extensive experiments on longevity in spores and sporidia of *Cronartium ribicola*. Two new species of leaf spot of maize, a disease distinct from leaf blight, were described by Charles Drechsler. E. F. Gaines discussed the results of ten years of breeding for bunt resistance with 500 varieties and selections of wheat and for smut resistance with 208 varieties of oats. Resistance in wheat is recessive, while in oats it is dominant, and multiple factors are most important in explaining inheritance of disease resistance in these cereals. A. J. Riker gave two papers throwing additional light on the thermal relations influencing the development of crowngall and the cell behavior in infected tissue. Symptoms distinctly different from tobacco mosaic can be obtained on tobacco plants inoculated with extract from mosaic potato leaves, according to James Johnson. Cane plants selected for resistance to mosaic have maintained resistance for three years. C. W. Edgerton's paper declares this to be a probable case of acquired resistance. C. M. Tompkins gives some interesting data on the effect of temperature on masking mosaic. Temperatures above 24 degrees C. are unfavorable to mosaic development. A new piece of apparatus known as a fluometer has been devised for measuring water flow interference in diseased stems by I. E. Melhus, J. H. Muncie and Wm. T. H. Ho.

C. W. Edgerton, of the Louisiana Experiment Station, was elected president and M. F. Barrus, of Cornell University, vice-president.

The American Society of Plant Physiologists

President, Charles A. Shull.

Secretary, Wright A. Gardner, Alabama Experiment Station, Auburn, Ala.

(*Report by Wright A. Gardner*)

The first annual meeting of the American Society of Plant Physiologists was held at the fifth Washington meeting of the American Association for the Advancement of Science, from December 29 to January 1. The meeting was well attended, and much important business connected with the future development of the society was transacted. Two program meetings were held and four business meetings, and a